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Editor—HENRY E. SIGERIST

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CARL LUDWIG AND HIS AMERICAN STUDENTS

GEORGE ROSEN, M. D.

I

INTRODUCTION

American medicine was derivative in its origins. It is evident enough that in the beginning it was an integral part of a culture brought from Europe by the first settlers and was very much influenced in its development by European sources. D. R. Fox (1) in an essay, "Civilization in Transit," has traced four stages in any such cultural transfer: first, a few, educated in the mother country, emigrate to a colony or pioneer settlement; then, the native born youth is drawn back to the mother country for training; in the third stage schools are established in the newly settled country but the teachers and numerous students are still trained abroad; the fourth stage is present when the educational establishments of the younger country become adequate.

Research bearing directly on this fascinating aspect of the development of medical science in the United States has been rather meager. The influence of the Edinburgh school on the numerous American students, who were attracted by it in the 18th century, has received a judicious and philosophic treatment at the hands of Dr. J. Gordon Wilson of Chicago. Sir William Osler dealt at length with the influence of the French school in the first half of the 19th century, particularly of Louis on his American disciples.

There has been no attempt however to study the influence of the various German schools on the American students—and through them on the development of scientific medicine in America—who in the latter half of the 19th century flocked to Germany to learn pathology from Virchow, bacteriology from Koch, and physiology from Ludwig. In this essay, I shall attempt to analyze the influence of Carl Ludwig on the American students who passed through his laboratory and to show the decisive role played by these men in the development of modern American medicine.

II

Carl Ludwig

Medical evolution from ancient times to the present can be characterized by the increasing objectivity of medicine as a science. The tremendous difference between ancient and modern medical science lies not so much in the more complete knowledge, nor in the increased diversity of method, nor in a broader point of view, but rather in the basic concept which binds this entire structure together, the idea of scientific method. The medicine of the Hippocratic writings, of Galen, of the medieval Arabic, Christian and Jewish authors was based mainly upon aprioristic theories and this condition existed well into modern times. The older medical thinkers determined the truth and value of a theory by the dialectic acumen of the proof, the number and reputations of the cited authorities, the boldness of the conception and the conclusive logic. The rationalistic superstructure helped often to cover a factually barren foundation. The nineteenth century saw a complete change in this respect. Today in the medical sciences nothing is considered to be truly, scientifically proven which has not stood the test of experience and experiment. None of the medical disciplines exhibits this transformation better than the science of physiology. A teleologic natural philosophic subject concerned with "de usu partium" (1) became, as Haller named it, "Anatomia animata" (2), an experimental science intimately acquainted with the mathematical spirit and methodology of physics. This reform was initiated by the French physiologist Magendie (3). His slogan was: "La médecine est une science à faire." No one however was more instrumental

in the modern development of physiology than Carl Ludwig (4). The history of his life coincides roughly with the period of progress in physiology during the latter half of the nineteenth century.

Of the outstanding physiologists of the last century it is recognized by the medical world that Ludwig was the greatest teacher and the one who unquestionably exerted the most fruitful influence. Ludwig was born on December 29, 1816, at Witzenhausen, on the Weser, near Cassel (5). The period during which Ludwig spent his youth was one of reaction. The long and devastating Napoleonic wars and the struggle against foreign invasion had come to a close. Exhausted and existing primarily as a group of petty principalities bound together by a nebulous political and national solidarity, the result was that the German people were forced to submit for an extensive period to the rule of a brutal military regime.

Ludwig's father had participated as an officer in the struggle against Napoleon, had been wounded and forced to turn his back on a military career. Being in favor with the Elector of Hessen he was appointed to the office of "Rentenmeister" at Hanau (6). Here Ludwig spent his childhood and youth.

After completion of the course at the gymnasium in Hanau he was sent to the University of Marburg. Here he was a stormy petrel among the students. A deep scar on his upper lip bore witness in later years to his youthful turbulence and participation in duels. The result of various disputes with the university authorities, perhaps because of political activities, was that Ludwig was compelled to leave Marburg. Ultimately, he returned there after having studied at Erlangen and at the surgical school at Bamberg (7). In 1839 he took his doctor's degree at Marburg (8). Ludwig continued his studies and two years later he became prosector of anatomy under Professor L. Fick. In 1842 he obtained the venia legendi with a thesis on the mechanism of renal function.

As I pointed out above this period was one of repression and consequently of unrest. This holds true for all kinds of thought, scientific as well as socio-political. Brutal repression was accompanied by romanticism, even as today, and the thinkers turned from the materialism of the 18th century to romantic idealism. The systems of Schelling, Fichte and Hegel ruled the thought of the time. Medical science did not remain immune to the unwholesome influ-

ence of these doctrines. The scientific life which unfolded itself in consequence was extremely barren. Particularly the medical sciences were dominated by the superficial analogies and aprioristic conclusions of Schelling's "Naturphilosophie," which diverted the mental energy, that should have gone into the careful, scientific examination of facts, into the domain of poetic fancy and idle speculation (9). Magendie had initiated the reform in medical science (10), particularly in physiology, and in Germany it was also a physiologist, Johannes Müller, who carried on the struggle to reform the medical sciences. Müller, although he threw speculation overboard, and pursuing the methods of the exact sciences, recognised that animate organisms are governed by the same forces which control inorganic matter, still assumed the presence of a life-force, which caused protoplasmatic reactions to be different from inorganic processes (11). The overthrowal of this vitalistic doctrine was one of the tasks to which Ludwig, together with the pupils of Johannes Müller, Du Bois-Reymond, Helmholtz and Brücke, was to devote himself.

The dissertation, which he offered for admission to medical faculty at Marburg, in 1842, exhibits already the characteristic features of his future work, the desire to explain vital processes in terms of physics. In his thesis: "Beiträge zur Lehre vom Mechanismus der Harnsecretion," Ludwig develops a physical theory of renal secretion (12). The structure of the kidney glomeruli seemed to suggest that the first step in renal excretion would be a liquid diffusion through a membrane due to a difference in pressure between the two sides. Ludwig sought to support this work by physico-chemical experiments, published as: "Über endosmotische Aequivalente und endosmotische Theorie" in 1849 and 1856 (13). These researches were the foundation of our knowledge on diffusion through membranes.

In 1846 Ludwig became Extraordinary Professor at Marburg and here he worked until 1849, at which time he received an appointment as Professor of Anatomy and Physiology at Zürich. Industriously Ludwig continued his work during this period. The circumstances under which he worked in these years conditioned the bent of his later thought and allowed his innate faculties to develop in such a manner that he became one of the greatest experimenters in the history of physiology. We must not forget that as prosector

he was required to demonstrate anatomy in the dissecting room. Thus on the one hand he was an anatomist, on the other a student of physics conversant with the newest developments and methods of this science. The physicist Bunsen, who taught chemistry and physics at Marburg and with whom Ludwig was intimate, influenced him a great deal (14). The combination of Ludwig's ingenuity, resourcefulness and knowledge of physical science enabled him to revolutionize the methodology of physiology. At the time he was entering upon his work there was an almost complete lack of physiological laboratories and instruments. In the course of his experiments Ludwig invented numerous methods and instruments. In 1847 he described one of the most interesting and important of his contributions. This was the kymograph. His description of it appeared in a paper published in Müller's Archiv: "Beiträge zur Kenntniss des Einflusses der Respirationsbewegungen auf den Blutlauf im Aortensysteme." The report of the instrument appeared in classically, simple words: "Um durch ihn (Poiseuilles' Quecksilbermanometer) nun gute Druckzahlen unter allen Umständen und zugleich Zeitbestimmungen für die Dauer und Folge der einzelnen Druckgrössen zu erhalten, setzt man auf das Quecksilber einen stabförmigen Schwimmer, versieht ihn am oberen Ende mit einer Feder und lässt diese Schwankungen auf eine Fläche zeichnen, welche sich mit gleichförmiger Geschwindigkeit an der Feder vorbeibewegt. Auf diese Weise erhält man Curven, deren Höhe ein Ausdruck für den Blutdruck, deren Breite eine Bestimmung der Zeit ent hält"(15). ("To obtain reliable figures under all circumstances by means of it (referring to Poiseuilles' mercury manometer), and at the same time, time determinations for the duration and course of the different pressures, one places a rod-like float on the mercury, puts on the upper end a writing point, and lets it draw the variations in pressure on a surface, which moves by the pointer with a constant velocity. In this way one obtains curves, the height of which is a measure of the blood pressure and the width an indication of the time.") A similar contrivance had been employed by Poiseuille ten years earlier. Thomas Young and through him other physicists had used graphic methods to record their results. Ludwig's Kymograph however paved the way for a wider application of this method, and in its subsequent modification by Marey

and Chauveau it is known as the " graphic method " (16). His study of the circulation led Ludwig to other inventions, important ones being the stream-gauge (Strom-Uhr), the mercurial blood pump, and the method of maintaining a circulation in isolated organs.

The fundamental goal which Ludwig strove to attain was to be able to explain vital phenomena in terms of mechanics. In this respect he was in complete agreement with du Bois-Reymond who in 1848 wrote: " es könne nicht fehlen, dass dereinst die Physiologie ganz sich auflöst in organische Physik und Chemie " (17). Great importance in the development of his thought must be attached to a visit to Berlin in 1847 in the course of which Ludwig made the acquaintance of Helmholtz, Brücke and du Bois-Reymond, men who were to be his life-long friends and companions in the creation of modern physiology (18).

Ludwig himself has best described his development in these years in a letter written in 1849: " Nachdem ich sechs Jahre als Student allen alten Kohl der Mediziner durchgearbeitet und endlich eingesehen hatte, dass er innerlich haltlos war, begab ich mich vor nun schon zehn Jahren an ein Studium der Naturwissenschaften. Als ich das in meinem 23. Jahre begann, konnte ich keine regula de tri rechnen, ich hatte noch nie einen Destillierapparat gesehen und das Mikroskop nur aus der Ferne betrachtet. Ohne Lehrer, durch einen unermüdlichen Fleiss brechte ich es nach einigen Jahren dahin, dass ich eine organische Analyse machen, das Mikroskop handhaben und Fischer's Physik lesen konnte, und so habe ich mich Schritt vor Schritt, zuletzt durch die Berührung mit Bunsen und unserm Mathematiker Stegmann und in stetigem Hinschauen auf Brücke und Dich (du B.-R.) auf meine gegenwärtige Lage gebracht " (19).

In 1849 Ludwig became Professor at Zürich. His time was taken up with dissection and lectures. He complains to du Bois-Reymond that: " Mir selbst bleibt nur sehr wenig Zeit zu eigenen Arbeiten und in der Beziehung ist die fröhliche Zeit von Marburg vorüber; wenn man 4-5 Stunden im Präpariersaal und zwei tägliche Kollegien lesend zugebracht hat, so kehrt man meist todmüde nach Hause, so dass ich selbst am Lehrbuch keinen Strich geschrieben, seit ich in Zürich bin " (20). In spite of this he continued his researches on the physiology of secretion showing that the secretion of the salivary glands is dependent on nerve stimulation and not on

the blood supply (21). At the same time he continued the writing of his "Lehrbuch der Physiologie." In 1855 he was appointed to the professorship of Anatomy and Physiology at the Josef's-Akademie at Vienna. The next year the second volume of his "Lehrbuch" appeared. (The first volume had appeared in 1852.)

Before considering the book I wish to quote the prophetic words du Bois-Reymond wrote in a letter to Ludwig in 1852, the year of the appearance of the first volume of his book. He said: "Allein du musst nicht vergessen, dass es in den Wissenschaften Epochen gibt, wo es sich just so sehr lohnt, ein Lehrbuch zu schreiben, als die bedeutendste eigene Arbeit zu machen. Eine solche Epoche sehe ich jetzt in der Physiologie. Sie ist begründet dadurch, dass unsere Schule die Dinge in einem ganz neuen Lichte sieht. Auch wenn kein neues Material vorläge, welches die alten Lehrbücher noch nicht enthalten, schon bloss das Alte in unsere Denkart zusammen gestellt, giebt eine ganz neue Wissenschaft. Und in fünfzig Jahren von hier wird als dann vielleicht diese Epoche durch das Erscheinen Deines Lehrbuches charakterisiert" (22).

Beyond a doubt du Bois-Reymond was correct in his appraisal. It was the first modern text book of physiology and any one comparing Ludwig's work with a recent text will be struck by the similarity of arrangement and presentation. The book was meant for students and made no attempt to define life or to elucidate "biological principles" or "vital forces." All explanations of living processes were couched in terms of physical and chemical science. The whole book was an expression of Ludwig's scientific purpose, which he summed up in the introduction. Defining physiology he wrote: "The task of a scientific physiology is to determine the functions of the animal body and deduce them as a necessity from its elementary conditions" (23). He considers the ultimate aim of physiology the resolution of these conditions into their simplest elements and to show that the "forms of activity arising in the animal body must be the result of the simple attractions and repulsions" which result from "the coming together of those elementary objects" (23).

In Vienna Ludwig carried on the work begun in Marburg and Zürich. At this time he was particularly concerned with investigations of the blood gases. In 1859, in a paper published by his pupil

Setschenow, he described his invention of the mercurial blood pump. This instrument enabled him to separate from a given quantity of blood taken directly from the blood stream, the gases which the blood sample contains (24). In the next 35 years Ludwig and his pupils were to develop these researches in innumerable directions comprising the entire subject of respiratory exchange.

When the new chair of physiology was instituted at Leipzig in 1865, Ludwig was offered the occupancy and accepted. His first problem was the planning of a physiological institute. Ludwig was also a pathfinder in this field and created at Leipzig a model for others. It is probable that Liebig's chemical laboratory at Giessen and the accommodations of his colleague at Marburg, Bunsen, served him as examples. The institute which was erected under Ludwig's supervision was however based on his broad conception of physiology. This complex science requires numerous auxiliary forces as compared with the more uniform sciences of physics and chemistry and he emphasized this in his speech at the dedication of the institute in 1869. (Die Physiologie ist) "aus der Anatomie hervorgewachsen, und bei dem Physiker und Chemiker in die Lehre gegangen, aber sie hat zu den Werkzeugen, die sie sich dort zu eigen gemacht, neue gefügt und mit beiden Neues gefördert" (25). The manifold problems of physiology can be elucidated only by combining a study of the anatomy of the organ with a knowledge of the physico-chemical changes which take place during it's function. In view of this fact which was fundamental in Ludwig's work he created three divisions in his laboratory: an anatomical division including histology, a physical and a chemical division. It will be worth our while to describe the laboratory and the atmosphere which characterized it in detail, as it will help us to understand the conditions under which Ludwig's pupils worked.

The building, which housed these departments, was constructed in the form of a capital E (26). Lombard says that "the main portion was arranged and equipped for the study of physiological processes from the physical side, one wing was devoted to histological work, the other to physiological chemistry. The lecture room, closely connected to the main part of the building, occupied the space between the wings" (26).

Students upon entering the laboratory sensed the spirit which

pervaded it. It was an atmosphere compounded of equal shares of hard work and enthusiasm. There was a division of labor based upon Ludwig's innate capacity to judge the leanings and capabilities of his students. He was able to find something to do for each worker and he cooperated in the experimentation and in the publication of the final results.

"Every morning he visited the tables of the different men and discussed with them the next step to be taken . . . , or he would take them into his private room and critically discuss the methods employed, making suggestions as to the direction in which new and more effective methods could be sought, carefully go over the curves and other data already obtained and the inferences to be drawn from them. This was not done offhand, for each night when he left the laboratory he carried to his rooms above, records and protocols of investigations in progress, for careful study" (27). There is hardly a doubt that Ludwig's significance as a teacher should be rated as high as his achievements in the field of science. His conception of a teacher's duty was a high one and he expressed it in a letter to his pupil Warren P. Lombard. "Destiny has conferred on us professors the favor of helping the responsive heart of youth to find the right path. In the seemingly insignificant vocation of the school master there is enclosed a high, blessed calling. I know no higher" (28).

These facts help us to understand why men from foreign lands flocked to his laboratory. His laboratory was always full of students and Kronecker mentions the fact that it was not rare to find only one German in a group of 9 or 10 workers, all the rest being foreigners. Osler in a letter written in 1884 summed up his impression as follows: "Probably the most notable figure in medical Leipzig is Professor Ludwig, Director of the Physiological Institute and the Nestor of German physiologists. Indeed he has a higher claim than this, for when the history of experimental physiology is written his name will stand pre-eminent with those of Magendie and Claude Bernard. . . . He has the honor of having trained a larger number of physiologists than any other living teacher; his pupils are scattered the world over, and there is scarcely a worker of note in Europe — bar France — who has not spent some time in his laboratory" . . . (29).

The entire work of the institute, experimental as well as pedagogic, is characterized by its complete unity of purpose. Ludwig's lectures illustrate this perfectly. They were usually given at four in the afternoon after having been well prepared. Ludwig addressed his lectures to the mature student and all those working under his direction at the institute attended them. The undergraduates had a difficult time and most of them attended the lectures several semesters before being examined. Ludwig attended to his lectures in the same thorough, enthusiastic manner which characterized all his work. He prepared the experiments, which were to illustrate his lecture for that day, himself, assisted only by his laboratory mechanic, Salvenmoser. The interest of the audience was usually centered upon these experiments, as they were very often the result of work done recently in the laboratory. All this created among the students a feeling that this was a place where things were being done; that "this is not hearsay; the evidence is here on the spot" (30).

In 1874 on the 25th anniversary of his professorship, students who had worked with him at Marburg, Zürich, Vienna or Leipzig presented him with a collection of original memoirs. On his 70th birthday in 1886 a second Festgabe was presented to him by his former pupils. He died at Leipzig in 1895.

Ludwig belonged to that remarkable group of German physiologists who from its small beginnings in the latter half of the 19th century, created the modern science of physiology. His life is to a great extent an integral part of the history of that science. His work will remain associated in the history of medicine with the physico-chemical bent of recent physiology and its tendency to approach the exactness of the natural sciences through mathematical formulation. Ludwig's creation of a modern physiological laboratory and his extraordinary influence as a teacher place him in a unique position among the masters of physiological science in the 19th century. His relations with his pupils were based upon the fundamental thought underlying his pedagogic activity: "Pass it on" (31). A study of Ludwig's American students illustrates advantageously the influence that all of these factors exerted on the men who worked with him.

III

American Students

Carl Ludwig influenced American medicine most decisively through his pupils. A much more extensive study than this essay would be necessary to describe in detail how these men, who worked with him at various periods, applied the spirit and methods, which they learned from Ludwig, to their work in America. In doing so they gradually effected a change in the teaching and pursuit of medical science in this country which can only be called revolutionary.

The majority of the Americans who studied under Ludwig did so at Leipzig. There is a record however of two students at Marburg. In the Festgabe of 1874 they are listed as *Ludwig Gerling*- Dr. med. pract. Arzt-, Augusta, St. Charles County, Mo. who was with Ludwig from 1843-1847; and *Gerau,* Dr. med. Arzt- New York, who was at Marburg in 1845-46. There are no further records of these men and from their names I would suppose that they were Germans who may have emigrated to the United States, perhaps after 1848.

Ludwig had no American students at either Zürich or Vienna and it was only after he had been at Leipzig for a few years that the first student from this country came to him. Some of the most eminent men in the history of American medicine passed through his laboratory. Henry P. Bowditch, Charles S. Minot, William H. Welch, Franklin P. Mall, John J. Abel, Frederic S. Lee, and Warren P. Lombard were there at some time during their studies abroad. For the purpose of this essay I shall limit myself by referring somewhat fully to the careers of those who have had a major and determining influence on the development of medicine in this country with special reference to medical education and research.

Henry Pickering Bowditch

The history of institutes for the experimental pursuit of physiology and the other medical sciences is little more than a century old. Gruithuisen in 1812 had published an article advocating physiological institutes. Twelve years later, in 1824, Purkinje established the first physiological institute in Breslau. In the course of the next

forty years such laboratories were established sporadically, particularly in Germany. According to Welch (1), the one which exerted the greatest and most fruitful influence was unquestionably that of Ludwig in Leipzig.

The systematic development of such institutions in the United States has an even shorter history and it was in this field that the influence of Ludwig was first to make itself felt. The first laboratory for experimental physiology, and one might better say for experimental medicine (2) in America was founded by a pupil of Ludwig, Henry Pickering Bowditch in 1871.

Bowditch was born in Boston on April 4, 1840, as the son of Jonathan Ingersoll Bowditch, a merchant with a scientific turn of mind (3). His youth was uneventful and was typical of that of the youth of his class in Boston during the middle of the nineteenth century. In 1857 he entered Harvard College without any intentions however as to a medical career. Four years later on the eve of the Civil War we find Bowditch entering the Lawrence Scientific School in Cambridge to study chemistry and natural history. The outbreak of war interrupted these studies and from 1861 to 1865, he saw active service as an officer in the Union Army. Upon cessation of hostilities Bowditch resigned his command and resumed his studies, in comparative anatomy, at the Lawrence School. At this time he came under the influence of Jeffries Wyman, who directed his work in comparative morphology (5). Wyman made a deep impression on Bowditch and stimulated the development of his scientific thought. His decision to devote himself to medical studies dates from this period and in 1868 he received his M. D. degree (6).

The very same year saw his departure for Paris and further study. Indecision as to the further course to be pursued marks this period of his development. He followed the clinics of Charcot and Broca, but due to the failure of Brown-Sequard, with whom Bowditch had planned to work, to provide a place for him, he turned his attention to physiology and histology. These studies led him to Claude Bernard and Ranvier and slowly but surely he began to devote his attention to the scientific basis of medicine (7).

The turning point in his evolution came in March 1869 when he met Willy Kühne (7A). Bowditch requested the German physiologist to lay out a course of physiological study. The latter suggested

some months with Max Schulze, a year with Carl Ludwig in Leipzig to be followed by a final period with Virchow and then with Helmholtz in Berlin. Bowditch was seized by the attractiveness of this suggested plan and decided to embark at once upon the undertaking.

After spending a few months studying histology with Schulze and Rindfleisch in Bonn, he arrived at Leipzig in September. Ludwig had entered upon his professorship in Leipzig in 1865 and in the intervening four years students from most of the European countries had found their way to him. Among them at this time were Lauder-Brunton, Lankester, Mosso, Kronecker and Ustimowitch, and Bowditch became a member of this group when he entered Ludwig's laboratory. He was seized at once by the spirit and enthusiasm which pervaded the institute and entered into the activities with heart and soul. Ludwig's presence, his thoughts and actions were a source of delight and inspiration to the young American. Ludwig had invented his contribution to the graphic registration of physiological reactions, the kymograph, in 1847. The recording of the time and the interval of stimulation were however still done by hand. Among Bowditch's first achievements was a mechanism to register it automatically. A letter of Nov. 7, 1869 relating this event bears witness to the esteem and regard in which Ludwig was held by his pupils, and exhibits the enthusiasm he was able to awake in the young men who came to him. " Prof. Ludwig is a very amiable and agreeable man. He must be between fifty and sixty years old but he retains his youthful enthusiasm and a remarkable faculty of finding pleasure and amusement in trifling matters. I arranged a little apparatus yesterday attached to a metronome for the purpose of marking time on a revolving cylinder covered with smoked paper (an instrument much used in various physiological experiments), and it was real fun to see how delighted the professor was with it " (8).

Bowditch continued to work in the physiological laboratory and this experience had a profound and stimulating effect on his scientific thought and interests. The Leipzig laboratory as I have already described was characterized by the simultaneous pursuit of various problems. Anatomical, chemical and physiological researches were carried on side by side. The atmosphere was congenial as Bowditch

records, finding " the exhaustive way in which questions are treated by the German investigators " . . . " very satisfactory " and differing " very strongly with the French method " (9).

Meanwhile conditions at home had been changing. Charles W. Eliot became a president of Harvard University in May, 1869 and began to reform the plan of education at this institution (10). Until 1865 instruction in physiology was combined with anatomy in the person of Oliver Wendell Holmes, who contented himself with delivering a meager smattering of physiology to the medical students in the form of a few lectures at the end of the semester and in pithy remarks on function during his anatomy lectures (11). In December 1869 Bowditch received an offer from President Eliot to lecture on physiology at Harvard during the second term of 1870-71. He required however six more months of this valuable period in Germany and declined the proposition. For the present things remained as they were and Bowditch continued his investigations. As we have seen Ludwig's ideal was to attack physiological problems by physical methods and to explain the reactions in terms of chemistry and physics. His students learned the manifold problems of physiology and the technique of experimentation. For students with an aptitude for invention, the employment of physical methods gave full play to their special faculties. This was true in the case of Bowditch and the construction of the " Bowditch clock," a contrivance to record various time periods, was a second product of his year at Leipzig. Cannon says that Ludwig's attention was first directed to young Bowditch's capabilities by these inventions (12).

His investigations during the year in Leipzig were directed to the physiology of cardiac muscle and of the heart proper. The results of these researches were published in two separate papers after Bowditch had returned to Boston. His work on the heart developed in two directions. One group of experiments was concerned with the neural control of cardiac action, in particular with the influence of varying arterial blood pressure on the interference between accelerator and inhibitor fibers of the heart. The second line of investigation was devoted to an examination of myocardial irritability; it led to results which have become classical contributions not only to the physiology of the heart muscle but also of skeletal muscle and nerve trunks (13).

In August, 1871 Ludwig received the results of Bowditch's research in the form of an " essay " and " seemed quite well pleased with it." When he left Leipzig a few days later to return to Boston, Ludwig accompanied him to the train and took leave of him " very affectionately " (14). Bowditch was returning to occupy the position of assistant professor of physiology at the Harvard Medical School. In April 1871 he had been invited by President Eliot " to take part in the good work of reforming medical education " (15). The invitation was accompanied by an offer of an assistant professorship. Bowditch accepted unconditionally.

While yet in Leipzig he had been buying apparatus to be used in a laboratory of his own on his return to America. There was however no laboratory awaiting him on his return. The Medical School was housed in an old building which was decidedly crowded. However the attic was renovated and in two small rooms Bowditch arranged the apparatus which he had acquired in Germany, thus inaugurating the first physiological institute in this country. His stay in Leipzig had exerted a profound influence on Bowditch's scientific development and through his laboratory American students began to learn the methodology and ideals of Ludwig's school.

This entire development is a very apt illustration of Fox's third stage of a cultural transfer (16). Bowditch started during the third phase and was instrumental in starting the fourth stage. We will find this to be true of other American pupils of Ludwig such as Drs. Welch and Mall.

The laboratory in Boston developed successfully and in 1877 William Osler in a study of the Harvard medical Faculty points out particularly " H. P. Bowditch's course in practical physiology " (17). At this time in a letter to E. A. Schäfer, Osler writes: " You will be interested in Bowditch's and Warthin's laboratories, the only good ones on the continent" (18).

Productive scholarship was the alpha and omega of the institute from its very inception. Next in importance was the attraction and training of interested workers. The foreword to the first group of reports to appear under the imprimatur of the institute expresses this thought. The papers are presented " not from any exaggerated idea of their value and importance, but with the hope that by calling attention to the facilities offered in the laboratory for original

research a greater number of workers may be encouraged to attempt the investigation of the many physiological problems now pressing for solution" (19).

The activities of the laboratory did not belie the announcement of its purpose. The researches pursued there were manifold and its hospitality knew very few boundaries. In this manner Bowditch transmitted to a new generation the stimulating curiosity for investigation that he had imbibed under Ludwig's tutelage. The two attic rooms harbored researches in practically every phase of experimental medicine. Investigations in physiology, general biology, pharmacology, experimental pathology, psychology and experimental surgery were being carried on there soon after its foundation. Charles S. Minot, G. Stanley Hall, William James, Walter B. Cannon and Warren P. Lombard worked there. H. C. Ernst worked in bacteriology.

Bowditch worked along some of the lines of investigation he had pursued in Leipzig and devoted himself to many new interests. He invented ingenious apparatus in the course of his experiments. A new type of plethysmograph to record the volume variations of organs, a cannula enabling one to observe the vocal cords and a new induction apparatus, enabling one to obtain fine variations in the intensity of the induced current were some of the inventions in the course of his work. On the basis of his work in Leipzig Bowditch enunciated the "all-or-none law" which states that cardiac muscle responds to a stimulus with a maximal contraction or with none at all. He remained interested in cardiac physiology and in the Harvard laboratory he demonstrated the fact that, when the apex of the ventricle in the frog is isolated from its base and kept in suitable media, it shows no spontaneous rhythmic activity although it is still responsive to external stimuli.

The neural regulation of the vascular system was another line of investigation followed up in the Harvard institute. Bowditch in collaboration with Minot had demonstrated in 1874 that the effect of chloroform in depressing the vasomotor reflexes is much greater than that of ether (20). This was followed later on by a series of experiments which showed that by varying the rate and strength of the stimuli one could obtain either constriction of a vessel, or constriction and then relaxation and finally only dilitation. Similar

results were obtained by stimulating nerves which had been cut and allowed to degenerate. These experiments have been widely discussed. A variety of interests occupied the attention of Bowditch. At one time his scientific interest was captivated by the physiology of the nervous system. "What conception can one form of the physical or chemical changes which take place in those glistening bands which are for us the only channels through which knowledge of the physical universe can be obtained and which also enables us to impress upon the world around us the evidence of our conscious personality" (21). Bowditch's work exhibits very little speculation and deals almost exclusively with facts. Thus in work on nerves he demonstrated the practical indefatigability of the nerve, knowledge of which is fundamental to the understanding of the functioning of the nervous system (23).

All this while the reform of medical education at the Harvard Medical School had been progressing steadily. The status of physiology had been changed on the arrival of Bowditch in 1871. The original joint professorship of anatomy and physiology was divided. Bowditch was installed as assistant professor but he had complete control of the teaching activity of his department. In 1876, he received an appointment as full professor. The course in physiology now offered the medical students consisted of a model combination of lectures and demonstrations. I have already quoted Osler's remarks on these matters (24).

In 1883 Bowditch became dean of the medical faculty and occupied that position for ten years. During his tenure of office much was done to improve the medical curriculum and to bring it up to adequate standards of medical education. The regular required four year course of study, which is now commonplace in all medical schools was adopted and bacteriology was recognized as a regular and required medical subject (25).

Bowditch visited Ludwig at Leipzig seveal times and described the work at his institute (26). Once he demonstrated the amount of work done by ciliated cells in moving objects from one place to another, an effect which excited the interest of Ludwig and his assistants. Bowditch was active until his resignation in 1906. Five years later he died. Although in his later years he had been increasingly drawn away from active investigation, he had already

contributed his share towards the foundation upon which the superstructure of American medicine and medical education as we know it today was to be erected.

Charles Sedgwick Minot

The intellectual stimulation which Ludwig transmitted to his students was carried far and wide. They in turn passed it on to the young men who came to work with them. The effect of Bowditch's experience in Leipzig upon his students can be seen most advantageously in the development of Charles Sedgwick Minot (1).

The scion of an old New England family (2), Minot was born on December 23, 1852 in West Roxbury, Mass. While still a boy he evinced the greatest interest in natural history. This inclination towards the study of organic nature did not decline as he grew older but rather increased into an all absorbing study. In July 1868, at the age of fifteen, Minot became a member of the Boston Society of Natural History. The president was Jeffries Wyman who had directed the studies of Bowditch and it is likely that Minot also was influenced by him. Minot at once became an active member of the Society and in September he already submitted a brief communication to the Entomological Section.

In spite of his devotion to the study of natural history, he entered the Massachusetts Institute of Technology in 1868, choosing chemistry as his major subject. A combination of various circumstances forced Minot to take this step. At this time he was still under sixteen and so he was unable to meet the age limit of 18 years required for entrance to the Scientific School of Harvard. His entrance to Harvard College, where Asa Gray and Louis Agassiz headed the department of natural history, was blocked by the requirement of classical study for admission (3).

At the Institute Minot was much influenced by Edward Pickering, the astronomer, who taught physics there. Here he already showed his ability to construct apparatus. His first piece of work was a set for microphotographic work, which he employed in his work on insect anatomy. In 1872 Minot graduated receiving the degree of Bachelor of Science.

This entire period was important for his development. At this time he was occupied with scientific reading, particularly on the

subject of organic evolution, which was to leave an impress on all his later thought.

After graduation he matriculated at the Harvard Graduate School. Originally Minot had intended to work for his Sc. D. in natural history and had already done some work with Louis Agassiz. The establishment of the physiological laboratory by Bowditch in 1871 and the opportunities for work there, perhaps also personal connection with him, caused Minot to become Bowditch's first graduate student. It was an education for Minot to come under the influence of the methods and traditions of the Leipzig laboratory from which his teacher had only recently returned. Together they performed a series of experiments, the results of which were published in 1874 in an article dealing with "the influence of anesthetics on the vasomotor centers" (4).

Minot had heard so much about Ludwig and his laboratory from Bowditch that in 1873 he went to Leipzig. He obtained a position in Ludwig's laboratory immediately and began his work in physiology. Ludwig gave him the problem of CO_2 formation in active and resting muscle to work on, and the experimental investigations together with the results obtained appeared in 1876.

Minot's primary interest was not in the field of physiology and while working with Ludwig he also pursued investigations with Rudolf Leuckart, the professor of zoology at Leipzig. These researches were concerned with the anatomy and taxonomy of certain groups of worms. While working in physiology and zoology Minot did a good deal of scientific reading in German, particularly Baer's "Entwicklungs geschichte" and his "Unsere Körperform."

In 1875 he interrupted his studies at Leipzig to go to Paris. The attraction here was Ranvier and Minot spent a few months with him learning histological methods and applying them to the microscopic anatomy of the water beetle Hydrophilus. The remainder of his time in Europe was spent at Leipzig with the exception of a short stay at Würzburg where he studied histology with Semper. Here Minot studied Leydig's "Lehrbuch der Histologie" and bettered his histological technique. In the fall of 1876 he returned to Boston (5).

The candidacy for the Sc. D. degree was taken up again. The University required Minot to spend another year in resident study,

consisting of work in gross anatomy and in Bowditch's laboratory. Minot did little work in gross anatomy but he collaborated with Bowditch in a group of experiments on tetanus. Professor Porter describes these investigations as " ingenious, laborious, meticulous, a conscientious collection of crumbs left by those earlier at the feast " (6). At the same time he had published the results of his European studies in a number of articles. These papers dealt with a variety of subjects ranging from the classification of worms to descriptions of zoological research in Germany. When Minot presented himself for his final examination he offered these articles and his work with Bowditch instead of the required thesis. In 1878 he received the Doctor of Science degree.

The next two years were full of indecision and uncertainty. Minot had no position and so he spent his time in scientific writing. During this period the underlying aim of his future work began to take shape in his mind. This was to be an elucidation of the essential nature of life, a problem, which whether considered possible or impossible of solution, actually underlies all physiological and morphological study (7). Minot's work now was primarily morphological in character.

At the end of these two years he received an appointment as Instructor in Oral Surgery and Pathology in the dental school and also as lecturer on Embryology in the medical school. There was a good deal of opposition due to the fact that Minot was a zoologist and not a physician. The appointments were procured for him with the support of President Eliot and Bowditch. He was not however admitted as a member of the medical faculty.

His first class at the dental school consisted of four students who learned the technique of histological sectioning, the use of the microscope and heard lectures on dental histology and embryology. The course in embryology at the medical school consisted of a few lectures. By 1883 Minot had so augmented the number of his lectures that he received an appointment as Instructor in Histology and Embryology. At this time the medical school moved to a new building and Minot had a chance to put his own ideas on the subject of a histological laboratory into practice. The histology laboratory was situated in a well lighted room on the top floor of the building and furnished with 18 microscopes (8).

Minot's conception of the relative values of gross anatomy and embryology in a medical curriculum was at variance with that of his more tradition-bound colleagues. In his introductory lecture after being appointed instructor he maintained that the study of developmental anatomy would " enable the student who has been seeking his way through the mazes of adult anatomy by sheer force of memory to have a mental picture which is at once clear, interesting and correct " (9). At a somewhat later time he thought that " far more time is usually devoted to anatomy than is advantageous to the student " (9).

Minot had definite inventive capabilities and in 1886 he designed the rotary microtome, which is today a commonplace instrument in all histological laboratories. Ludwig's instrument maker Baltzer made the first microtome. It has since been superseded by more precise contrivances but it is still used for certain types of work, particularly in cutting serial sections of embryos (10).

In 1885 Minot had completed his studies on worms and insects. From then on he was concerned almost exclusively with vertebrate morphology. Two years later he was appointed Assistant Professor of Histology and Embryology. In 1892 he brought out his opus magnum, his treatise on Human Embryology. It was the result of ten years of work and attempted " to present a comprehensive summary of embryology, as it bears upon the problems of human development " (11). The Human Embryology received immediate recognition and His wrote in the preface to the German translation of 1894: " Minot's work is at present the fullest human embryology which we possess . . . and even after its contents in many parts shall have become superseded, it will retain its value as a bibliographical treasure house " (12). Any one who reads the book will see how well the author has marshalled an enormous array of facts and summarized an immense literature on the subject (13). Minot also published an Laboratory Textbook of Embryology in which pig embryos were so clearly described that they have come into common use as specimens in the study of mammalian embryology. Another important service which he rendered the science of embryology was the creation of his collection of over two thousand vertebrate embryos. These were sectioned serially and have been

used as a basis for numerous researches. The methods used in establishing this collection have served as models for other laboratories.

In his later years Minot was occupied with investigations leading towards a theory of life. He never completed all the studies he had planned but in 1908 he published some of his results in his book on "Age, Growth and Death." He died on November 19, 1914 (14).

Minot did not play a major role in the development of American medicine, but there is no doubt that he helped immeasurably to establish more correct ideas on medical education. The inclusion and development of histology and embryology as full fledged medical subjects and the teaching of these subjects not only by didactic discourses but to a great extent by practical, objective demonstrations under the microscope certainly tended to raise the standard of medical education in America. Thus considering his work in embryology and in medical pedagogy one may to a certain extent regard Minot as a forerunner of Franklin P. Mall.

Wm. H. Welch

In 1850 medicine in the United States was being practiced and taught in a provincial and sectarian manner. Medical education was in general inadequate. Anatomy was only a tool of surgery and pathology was taught by means of lectures. Students who desired further study went abroad, usually to France. French clinical medicine under Louis was then at its height and these American physicians brought back to their homeland the methods and ideas of the French school. American medicine had just entered the third stage of development according to the theory of development of transferred cultural elements which D. R. Fox has suggested. In the next seventy years this was to be changed, in great part due to the ideas and efforts of William Henry Welch.

Welch was born on April 8, 1850 in Norfolk, Conn. (1). His father was a general practitioner and on the paternal side he was descended from a family of physicians. His boyhood was uneventful, being passed in the small village where he was born. He was educated at the school of an uncle who prepared him for entrance to Yale College. Welch was sixteen years of age when he entered the college in 1866. He intended to study medicine but upon gradua-

tion he accepted a teaching position at Norwich, New York, where he remained for a year.

At the end of the year Welch gave up the job and matriculated as a medical student at the College of Physicians and Surgeons in New York. He soon regretted this step and evidently realizing the need for further scientific schooling before commencing the study of medicine he returned to Yale. With the intention of devoting a year to work in chemistry he entered the Sheffield Scientific School and the Yale Medical School. Here he came under the influence of two gifted men, teachers of chemistry. They were Oscar H. Allen and George Frederic Barker. The latter, particularly, directed Welch's interest to the work of Kekule, whose writings he learned to read in the German. What influence, if any, these men exerted on Welch's development is difficult to determine. At the Sheffield Scientific School he met T. Mitchell Prudden, who was to play a major role in the development of American pathology. In 1872 he re-entered the College of Physicians and Surgeons.

At this time the College was the leading medical school of the country (2). It will pay us to study the method of teaching in this institution in some detail as it will enable us to realize the enormous changes introduced by Welch in medical education and particularly in pathology. The College of Physicians and Surgeons was a part of Columbia University and had been in existence about 65 years (3). It was regarded as the leading institution of its kind in the country, and considering the period very justly so. The teaching facilities were considered good since the College possessed a roomy building on 23rd Street.

The greater part of the course consisted of didactic lectures. Whatever practical work was required of the students it was restricted to anatomy and medical chemistry, with the exception, of course, of the clinical instruction given at the College dispensary and at several hospitals in the city. Three years of attendance at lectures was required for graduation. Attention was centered upon making a practitioner of the student and the preceptorial system was employed to this end. From the preceptor the student obtained his practical training. This was accomplished by accompanying him on his rounds and assisting in the office of the preceptor.

There was no required autopsy course in pathology although

Francis Delafield gave a voluntary course in pathology with demonstrations. A chair of pathology had been instituted in 1847, but at this time it was still attached to medicine. The student had no opportunity to learn pathological histology or even to use a microscope (4).

A student with some ambition and initiative could find something to do outside the required course and Welch became prosector to the professor of anatomy. Under the influence of Delafield he also devoted a good deal of his time to autopsy work. Thus he had opportunity to acquire some technical skill and to exercise his powers of observation. In 1876 after a year's interneship Welch sailed for Europe making Strassburg his destination. At this time he had no intentions of becoming a pathologist by profession (5).

Waldeyer was the director of the Anatomical Institute in Strassburg and it was to him that Welch went to study normal histology. He considered this a necessary prerequisite for the study of pathological histology. The fact however that Welch spent part of his time in Hoppe-Seyler's laboratory working in physiological chemistry is of significance in showing that he was as yet undecided as to the path he was to pursue. Von Recklinghausen, an "Altmeister" of pathology occupied the chair of pathology at Strassburg and Welch sometimes attended his autopsy and demonstration course.

At the close of the summer semester he went to Leipzig, being attracted by the possibility of studying with Heubner. Welch had considered the field of neurology as a possible field to practice in later. Heubner had done a good deal of work on the diseases of the cerebral blood vessels and he published an important book on the syphilitic diseases of the cerebral arteries—"die leutische Erkrankungen der Hirnarterien," Leipzig 1874 (6).

Heubner was however not interested in teaching at the time Welch came to him and the latter began to look around for some other laboratory where he could work. Wagner was professor of pathological anatomy and Welch attended his courses and autopsies. The institute afforded an opportunity for a willing worker and Welch having aroused Wagner's interest received selected specimens of tissue for examination and for report.

At this time however Welch decided to study physiology. Ludwig's laboratory was a veritable Mecca for students of physi-

ology both in Germany and abroad. Welch applied for a position and was accepted. This choice of his was to play a very important part in his further development. Just as Bowditch in his time had been associated with a gifted group of students, so Welch was now to have a similar experience. At this period Kronecker was Ludwig's first assistant and Flechsig and Drechsel supervised the histological and chemical departments respectively. The most noted among the foreign students was Pavlov. Welch became intimate with most of these men and formed a lasting friendship with Kronecker (7).

Ludwig gave Welch the problem of demonstrating the neural and ganglionic elements in the auricular septum of the frog's heart by means of the gold chloride impregnation method. Welch discovered the ganglionic cells and demonstrated the fibers entering and leaving them. At the end of the semester a farewell supper was given and a model demonstrating Welch's work was exhibited (8).

All this work had only been a preparation for further study under Virchow, Welch having finally decided to devote his attention to pathology. His second consciously developed program was to be frustrated however, as the first had been. This time it was due to the influence of Ludwig, whose physiological mind had not fallen under the sway of Virchow's cellular pathology. A knowledge of the importance of function for the understanding and further growth of pathology caused him to convince Welch to change his program and to apply for a position with Julius Cohnheim in Breslau (9). Ludwig himself wrote to him recommending Welch.

This choice was to be the turning point of Welch's development and of the utmost importance in his influence on his students and on medical education. Cohnheim's manner of thinking was definitely functional in nature and he was not so much interested in morphology. As compared with Virchow, Cohnheim considered his subject from a dynamic point of view while the former's way of thinking was more static. Welch was the only American in the laboratory at a time when a group of men worked there who were destined to future fame. Among them were Paul Ehrlich, Carl Weigert, Lassar, Neisser and Ottomar Rosenbach. Welch was highly regarded by Cohnheim and by his colleagues in the laboratory. Salomonsen tells us that " Cohnheim knew well how to appreciate Welch and he recommended him for the professorship of

pathology at the Johns Hopkins University . . . " (10). In this atmosphere which was so congenial to his further development, Welch spent a semester. He attended the autopsies conducted by Weigert and worked at the problem which Cohnheim had assigned to him. The problem proposed was to investigate the pathogenesis of acute pulmonary edema. At this time Cohnheim was very much occupied with the preparation of his text book of general pathology and he left Welch strictly alone. The result was as Flexner says a " splendidly conceived and executed piece of experimental work " (11). Welch came to the conclusion that the causes of the condition were primarily mechanical and that edema developed because of a disproportion in the work of the right and left ventricles of the heart. The paper was published just as Welch had written it.

Welch during his stay at Breslau had an opportunity to witness Koch demonstrate the results of his anthrax researches before Cohnheim and Ferdinand Cohn. Salomonsen was doing work on ocular tuberculosis, Ehrlich and Weigert were beginning to use aniline dyes to stain microscopic sections and all this work no doubt left its impression on Welch's receptive mind. Cohnheim was very favorably impressed by the work of Koch and placed great hopes in the future development of the new science of bacteriology. This attitude was prevalent in the entire institute and Welch was very much influenced by it, so much so, that when he departed from Breslau, Cohnheim sent him to Vienna to visit Klebs. The latter was studying the bacteriology of endocarditis and he demonstrated to Welch the bacteria in the lesions.

The medical " grand tour " upon which Welch had embarked was not yet over and he decided to stay in Vienna for a while. Here his time was divided among a diversity of subjects (12). He spent some time taking courses on the diseases of the skin under Hebra, on neurology and psychiatry with Meynert and especially in studying experimental pathology with Stricker. Welch attempted to study embryology but soon gave it up. Prudden was also in Vienna at this time trying to study embryology and met Welch.

At the end of the year he left Vienna to return to Strassburg. He had prepared himself in normal histology and physiology and had studied pathology so that now Welch felt he was ready to work

with von Recklinghausen, who was outside of Virchow, the outstanding proponent of the latter's doctrines in pathology. Welch applied for and received a position.

Virchow and Cohnheim were at swords' points at the time over their respective theories of inflammation, more specifically over the origin of the pus cells. Cohnheim maintained that the cells were white blood cells, whereas Virchow claimed that they arose from sessile tissue cells. The fact that Welch had studied with Cohnheim impelled von Recklinghausen to assign him to a study of inflammation in the cornea of the frog. Welch came to no definite conclusion on the subject but he was repaid for his work in other ways. Von Recklinghausen was a stimulating teacher and discussed many topics in pathology with his American student. His educational pilgrimage now over, Welch returned to New York in the spring of 1878.

Welch was undecided as to the next step to be taken. He had no other means of supporting himself than by medical practice. He hesitated and his indecision was prolonged by " destiny in the form of Dr. Goldthwaites " (13). Appointments to the medical services of the Army or Navy and also to interneships in hospitals were then determined by the results of competitive examinations. To meet this situation numerous private " cram " courses existed and the practice was highly remunerative. Welch and Goldthwaites organized a course of this sort and the former stuck to it for three years. At the same time he also had some private practice.

As far as the teaching of pathology went, Welch was undecided. The teaching of the subject had not changed materially since his student days. American medicine was still provincial and cut off from the new ideas current in Europe. It was Welch who was to play a most important role in bringing American medicine into contact with the currents of thought prevalent in Germany and France.

At this time as if impelled by a deus ex machina, Welch received two offers to teach pathology. Dr. Francis Delafield at the College of Physicians and Surgeons proposed that he give a series of lectures on pathological anatomy during the summer semester. Welch declined however because he had no opportunity to open a pathology laboratory. Now he received an offer from a rival institution, Bellevue Medical College (14). The offer consisted of a proposition

" to build two small rooms over a hallway, which, added to another room," could be converted into a laboratory. This position had the very great compensation that it was near the dead house at Bellevue. Here Welch worked for six years and exerted a great influence on the teaching of pathology in New York. Students from all the medical schools came to his autopsies, pathological demonstrations and to work in his laboratory. When the College of Physicians and Surgeons proposed to set up a pathology laboratory and asked Welch to become its head, he declined but proposed his friend Prudden for the post. Welch in working for Prudden's appointment created another pillar of support for the new pathology in New York.

In these six years that Welch worked at Bellevue progress was being made in medical education. Annual sessions were increased from five to seven months and somewhat more emphasis was placed on practical work in the laboratory. The American medical schools however were still not up to the standard set on the Continent. The next few years were to bring a revolutionary change.

The Johns Hopkins Hospital was nearing completion. The next step was the establishment of the medical school provided for by Johns Hopkins. Welch was asked to guide the new enterprise, to become professor of pathology and also pathologist to the hospital. His great opportunity had come and he accepted (15).

Great efforts were made to keep Welch in New York but to no avail. One can truly say that this was fortunate for medical science in this country. As the hospital was still incomplete in the summer of 1884 Welch set out for another year of European study. Great changes had taken place since he had left Germany. Since Welch had seen the first demonstrations by Koch at Breslau the new science of bacteriology had developed at a remarkable tempo due to the impulse it had received from his work and that of Pasteur. The center of interest in medicine had shifted from the human body to its parasites. Welch realized the significance of these developments and set out to obtain first hand experience (16). To prepare himself for future work with Koch, he first went to Munich to study with Frobenius. Here he received a careful training in bacteriological technique. At the same time he worked with von Pettenkofer and pursued studies in animal pathology. We will realize the importance of these studies in evaluating his later work in public health and

hygiene. Acting under Koch's directions he left Munich in January and went to Göttingen to work under Flügge who exerted a lasting influence on his bacteriological work. This was followed by a short course lasting several weeks under the direction of Koch himself. The course terminated with a study of the cholera bacillus (17).

In September 1885 Welch returned to Baltimore to begin work. A temporary laboratory was set up on the top floor of the biology building. Newell Martin had offered two rooms for this purpose. This was only very temporary and a building which had originally been intended as a dead house was converted into a pathology laboratory. This building was only two stories high and was originally intended to be occupied only for a short time. The completion of the buildings for the medical school was however delayed and the temporary quarters became permanent. Later two more stories were added to this building to house the departments of anatomy and pharmacology. It was here that Mall was to start his work at Johns Hopkins (18).

The medical school was not opened until 1893 but in the years from 1885 until then productive work began to appear from Welch's laboratory. This period marks a turning point in the history of American medicine. As Bowditch had set experimental physiology in America on a new path so Welch was to do the same for pathology. By this time departments of pathology wherever existing in this country had already broadened out to include besides pathological anatomy also morbid histology. Sometimes experimental pathology and bacteriology were included. When Welch came to set up his pathology department at Johns Hopkins he included all these fields and employed all the knowledge and resources he had gathered in his educational pilgrimages. For this reason the pathological department at the Johns Hopkins University and Hospital was destined to influence the progress of medical education and research in the United States so profoundly. Medicine could now hope to develop as a science based upon a simultaneous symmetrical development of pathology, since the latter was in a position to employ not only its own methods and resources but also those of collateral sciences. Thus Welch proceeded along his revolutionary path. When he first came to Baltimore he found a group of enthusiastic workers ready to follow him. Among them were Councilman,

Halsted, Mall, A. C. Abbott, Reed and Nuttall. Some of these men were later to belong to the faculty of the Medical School.

Welch was asked to select the staff for the Hospital and also the faculty of the Medical School. Thus it was due to his influence that Osler, Halsted and Kelly were chosen in 1888 and 1889 for the Hospital and later when the Medical School opened in 1893 that Mall, Howell and Abel were picked to form part of the faculty of the Medical School.

In his own department, Welch began to work and teach before the medical school was completed. At first the teaching was informal but everything soon was organized and courses and lectures in general pathology, pathological anatomy and histology, and bacteriology were given systematically. At the same time research was carried on unceasingly. Welch discovered the gas bacillus and made important studies of diphtheria together with Flexner. As a result of his interest in animal pathology which he had retained since his studies in Munich, he did work of lasting value on hog cholera. His papers on thrombosis and embolism and on the structure of emboli all contain important factual contributions to medical science (19).

Thus in the work of Welch American medicine had at last come of age. Johns Hopkins had become a focus where all the forces of medical progress in America could gather. New requirements for would-be medical students came into being. Training in biological, chemical and physical science and also in French or German was required. Slowly but surely these ideas permeated the country and from then on American medicine had a basis which was the equal of the best European schools.

In 1917 Welch resigned from the chair of pathology in order to devote himself to problems of public health. As a result a new school of hygiene and public health was established (20). This was followed by a broader idea of public health control and by a new conception of its organization.

Nine years later, in 1926, Welch resigned from the School of Hygiene in order to pursue the study of medical history. For this purpose he founded the Institute of the History of Medicine at Johns Hopkins and devoted his time to seeking out and buying books for the Institute. He aroused an intense interest in the subject and

created a firm foundation for research in medical history in the United States. Welch became emeritus professor of the history of medicine in 1931. He died in 1934 (21).

Welch was one of the most potent forces for the advancement of medicine that this country has ever had. He was the central figure of a group of men who transformed medicine in America from an art practiced by rule of thumb to an exact science.

Franklin Paine Mall

"Five and twenty years ago anatomists in America were British in method and in spirit; they were easy going, each man following leisurely his own individual bent. Since that time a remarkable change has taken place; the number of laboratories in which the structure and development of the human body are taught and investigated have increased tenfold, the number of investigators has grown in a still greater ratio, in quantity and quality their anatomical proceedings and journals have come to rival those of any country in Europe. In effecting this transformation the chief credit must be assigned to one man—Franklin P. Mall" (1).

He was born on a farm near Belle Plaine, Iowa on September 28, 1862. His youth passed uneventfully. Mall received his early education in a boarding school near his home and then entered the department of medicine and surgery of the University of Michigan. During his student period he exhibited a strong desire to get his knowledge through his own efforts and through didactic lectures (2). In 1883 he graduated receiving the M. D. degree. Soon after he departed for Germany with the intention of working in anatomy. After a short period at Heidelberg he entered the laboratory of His at Leipzig. Mall's further development was to be decisively influenced by his contact with His particularly in the subject matter of embryology, which was to occupy his attention.

In 1885 His suggested that Mall work with Ludwig for a while (3). Ludwig's laboratory was always filled with students and Mall had to wait until a vacancy occurred. At last however the coveted opportunity came and he seized it eagerly.

Dr. Welch, who himself had worked with Ludwig ten years before, wrote to Mall congratulating him. "It is certainly the ideal and best thing for you. I consider it in itself an education to come

under the influence of such a man as Ludwig, to learn his way of looking at and attacking problems in physiology. I think that no better training is to be had than that obtained from Ludwig" (4). This period of work in Ludwig's institute was to be the other great factor which determined the bent of Mall's future activity. Sabin writes that "So great was the influence of Ludwig over his mind, character and future work that it is impossible to overestimate it" (5).

Dr. Welch has said that Ludwig's influence was "the stronger and more personal one. . . . I doubt if he ever had a student to whom he was more attached or who had made a more profound impression upon him than Mall" (6). Mall recognized his indebtedness to Ludwig and once said: "To that master I owe much—all."

Ludwig gave the young American the task of studying the villus of the intestine. While doing this work Mall learned the methods of injecting blood vessels and lymphatics which were to be so useful in his later work. It was also in studying the villus and the circulation of the intestine that he began to think in terms of structural units in the anatomy of organs. This stay with Ludwig gave a strongly physiological bent to his work and Sabin has said "that his work was physiology in the hands of one with an intense interest in structure" (8).

Dr. Welch first met Mall in Ludwig's laboratory in the winter of 1884-85. In 1885 Welch returned to head the Johns Hopkins Medical School and soon heard from Mall who desired to work there with him. As a result he returned to America in 1886 and came to Johns Hopkins as a fellow in pathology. Mall stayed there for three years. He was assistant in bacteriology and also carried on some experiments on the digestion of connective tissue by certain bacteria (9). In 1889 Clark University opened and Mall became adjunct professor of vertebrate anatomy. Three years later he was appointed professor of anatomy at the University of Chicago. When the Medical School of the Johns Hopkins University opened in 1893 Welch induced Mall to return to Baltimore as the head of the anatomy department (10).

When he arrived at the Medical School Mall found the conditions under which he was to work very simple. The department of anatomy was housed on the top story of the original pathology

building (11). With this limited space at his command he began to organize the teaching of anatomy, histology and embryology overcoming all the obstacles in his path. Slowly but surely an increasing and steady stream of work began to appear and to spread Mall's influence.

The effect of his work was evident in the field of medical pedagogy as well as in that of anatomical research. Mall based his entire method of teaching on the one fundamental principle of freedom in education—freedom for the teacher and the student. He felt that the student should approach his work in the spirit of scientific discovery. Without a doubt Mall felt that he was in this way continuing the work and transmitting the ideas he had experienced in action at Leipzig. He considered education to be a dialectic process in which both teacher and student played important parts, the preceptor stimulating the student and in turn being influenced by the latter's problems. Barker has said that " he was . . . a master of what is called ' elbow-teaching ' " and this is probably the most apt characterization of his method of teaching (12). Mall had an aversion to didactic lectures, recitations based on measured assignments of text book reading and laboratory exercises so designed that they left no place for independent activity on the part of the student. On entering the Medical School the students accustomed to such a regimen were of course all at sea and floundered at first. They began the study of anatomy by being assigned to dissect a certain part of the body using an atlas or a textbook for guidance. There was no one to tell the student when to stop, how to go about his work or just what to look for when dissecting. Mall's idea was that the student must be his own teacher and the laboratory was well provided with models and atlases for this purpose. Mall and his assistants supervised the work and in doing so Mall did most of his teaching. Under these circumstances his method of " elbow-teaching " came into full play.

According to Barker, " no student who has even been at his elbow will ever forget the character of this relationship — his trenchant comments, his startlings of the intellect and emotions, his humorous and sometimes satirical thrusts and above all, his earnest and sincere desire to make the neophyte an independent worker " (13).

Gertrude Stein has given us an interesting sketch of Mall as seen by one of his students. "She delighted in Dr. Mall, professor of anatomy, who directed her work. She always quotes his answer to any student excusing him or herself for anything. He would look reflective and say, yes, that is just like our cook. There is always a reason. She never brings the food to the table hot. In the summer of course she can't because it is too hot, in winter of course she can't because it is too cold, yes there is always a reason. Doctor Mall believed in everybody developing their own technique. He also remarked, nobody teaches anybody, anything, at first every student's scalpel is sharp and nobody has taught anybody anything" (14).

The result of his educational activity has been sumed up as follows. One observer says that "by his personal influence and example, by his pupils and disciples, and by reason of the inherent excellence of the Leipzig traditions, he has succeded in Germanising the majority of the dissecting rooms and anatomical laboratories throughout the length and breadth of North America" (15). Flexner believes that "he made such definite contributions to the higher education and training of anatomists and produced in . . . a few brief years so large a number of . . . capable teachers and investigators, as to mark a new era in the history of higher educational endeavors" (16).

The second major contribution of Mall to American medicine was more directly confined to the sciences of morphology and morphogenesis. His most important and lasting contributions were his investigations of monsters and the pathology of early human embryos, and the introduction into anatomy of the conception of the structural unit. Of these lines of research the latter was probably first in point of time. In Ludwig's laboratory while studying the intestinal villus, Mall began to think in terms of structural units and this theory reached its best expression in his work on the liver published in 1905. According to this idea organs are composed of histological units the size of which is determined by the size of the capillary circulation which connects the afferent artery with its corresponding vein. In normal organs these units are equal in size and may be grouped together into larger units as in the lobules of the liver. These units are also functional equals, a fact which is important for the consideration of the physiology of the organ.

These phenomena are governed by the laws of growth so that a unit, which grows so large that its capillary bed becomes abnormally large, divides into two units with the formation of a new arterial vessel (17). Mall also applied this theory to a study of splenic structure and with J. B. MacCallum he unravelled the muscular system of the heart. In the latter case he showed that the spiral muscle fibers of the heart in contracting work in the same manner as one wrings out a rag.

Sabin points out the fact that Mall enriched the science of anatomy with new methods and that he frequently acknowledged the influence of Ludwig in this respect (18).

As we have seen Ludwig exerted a great influence on Mall personally and also in his anatomical work but we must not forget that Mall had also studied with His and that he was also an embryologist. In his opinion human morphology could best be advanced by investigating the genesis of organs and of the tissues of which they are constructed. In 1891 he described with unparalleled completeness a 28 day old, normal human embryo (19). All the organ systems were carefully and painstakingly studied and reproduced by means of models. It was the first modelled reproduction of a human embryo ever to be produced in this country. In the course of this work Mall also discovered that the middle ear and the Eustachian tube arise from the first branchial arch. This was only the beginning of a line of work, in the course of which he laid the foundation for the science of organogenesis. In this work we find a mode of thought and action which closely resembles the unity of purpose which we saw in the work of Ludwig. Mall attacked the study of anatomic structure in the human body not only with purely morphological tools but he concentrated all the forces of embryology, histology, cytology on the solution of the problem. As a result he was able to trace the development of organs from their inception in the embryo to their full growth in the adult. Some examples of his work are his researches into the embryology of the intestinal loops. Here he was able to trace their growth up to their situation in the adult. These studies were based upon animal experimentation as well as dissecting-room experience.

In Mall's work on the development of the circulation we can best see the influence of Ludwig at work. At Leipzig he had learned the

technique of vascular injection and now he applied it to embryology. Thus the origin of the blood vessels and the lymphatics in the embryo could be studied and their spread during development could be closely and accurately observed. But Mall was not only interested in morphology and morphogenesis for its own sake; he was very much occupied with the architectonics of organs and the dependence of structure on function (20). In this field then, considering his training at Leipzig and the great personal attachment between Mall and Ludwig, we can well discern the influence of the latter at work.

Mall's attention was drawn at an early date to the study of human monsters and pathological embryos. For this reason he collected embryos of all kinds and even sent a circular letter of request to American and foreign physicians to increase his collection. As a result of this activity his laboratory became the largest repository for specimens of its kind in the world (21). The embryos were studied systematically and not only from a purely embryologic point of view but also from the angle of clinical medicine and social welfare. Such problems as the causes of sterility and abortion, the causes and frequency of occurrence of pathological embryos were all taken up and studied. Mall's first report on pathological embryology appeared in 1900 as a contribution to the volume of the Johns Hopkins Hospital Reports published in honor of Dr. Welch. In 1908 he published an extensive study of human monsters. T. H. Morgan's opinion was that " the recent publication by Mall on the causes underlying the origin of human monsters marks an epoch in the study of teratology in this country, for he has treated the subject with a breadth of view and a wealth of illustration rarely found in the handling of this complex question. Mall has brought to the task a profound knowledge of the older literature of the subject, an appreciation of the most modern results in experimental teratology, and a thorough familiarity at first hand with the subject of human monsters. The physician and anatomist are brought into close touch with work generally supposed to be outside their proper field; and on the other hand, the student of malformations in the lower animals will be made to appreciate the inexhaustible supply of human materials with which the anatomist and physician are familiar "(22).

Mall placed the subject of the production of human monsters on a scientific basis and removed it once and for all from the realm of

superstition and old wives' tales. He showed that with the exception of certain hereditary conditions such as polydactyly, monsters are caused by the effect of environmental factors on normal ova or embryos. In his paper on cyclopian teratomata he expressed the following opinion: " It seems to me that the studies based upon our collections of embryos, as well as recent investigations in experimental embryology, set at rest for all time the question of the causation of monsters. It has been my aim to demonstrate that the embryos found in pathological human ova and those obtained experimentally in animals are not analogous or similar but identical. A double monster or a cyclopian fish is identical with the same conditions in human beings. In all cases monsters are produced by external causes acting upon the ovum " (23).

In 1914 Mall was appointed director of the department of embryology of the Carnegie Institution at Washington. He was also a trustee of the Woods' Hole Biological Laboratory and maintained close relations with physicians and biologists. He had also been very active in founding the American Journal of Anatomy in 1901 (24). He died on November 17, 1917.

Mall was one of the great founders of scientific medicine in America. The significance of his work is two-fold. As a teacher he transmitted Ludwig's heritage, developing it in his own manner, and upholding in America the ideal of freedom in education: as a scientist he first pointed out the path which modern physiological anatomy has followed in this country and laid the basis for a broader conception of embryology and its problems to include organogenesis and clinical medicine. Without a doubt Mall was the greatest anatomist America has yet produced.

Conclusion

Besides the four men whose lives and work we have just considered, other Americans worked at Leipzig in Ludwig's laboratory. Such were John J. Abel, Frederic S. Lee and Warren P. Lombard. Despite their pre-eminence in their particular fields, it would lead us beyond the limits of this essay were we to consider their work individually, since none of them can be compared in their influence on the development of American medicine with Welch or Mall. It

suffices to point out the fact that their research work and teaching activity have demonstrated sufficiently the influence that the training with Ludwig at Leipzig had on them.

The careers of the men whom we have considered in this essay belong to a period in the history of American medicine which is now closed. They belonged to a transitional period and therein lies the importance of their work. There was one experience which all of them had in common and it is this which I wish to emphasize. All of them went abroad to study and passed through the laboratory of Carl Ludwig. Here they were to a greater or lesser degree influenced by the ideals and practice of the Leipzig school. A comparison of Ludwig's work with that of his American students will show us his influence on medicine in the United States.

Bowditch modelled his laboratory after Ludwig's and introduced the latter's method of instruction into America; Welch was influenced by Ludwig to study with Weigert and so in an indirect manner he contributed to the development of pathology; Ludwig exerted the greatest personal influence on Mall, who in his method of teaching and in the bent of his thought transmitted Ludwig's ideals to American medicine.

At a time when the tides of nationalism run high, it is of the greatest importance to emphasize the fact that science and culture cannot live in a narrow national environment but depend on the free interaction of ideas for development and progress. The development of American medicine in the third quarter of the nineteenth century and Ludwig's influence on this development through his American students illustrates this point perfectly.

BIBLIOGRAPHY

Introduction

1. Fox, D. R.—Civilization in Transit, American Historical Review, 32:753, 1927.

Carl Ludwig

1. Neuburger, Max—Die historische Entwicklung der Experimentellen Gehirn- und Rückenmarksphysiologie vor Flourens, Stuttgart, p. XI, 1897.
2. cf. ibid.

3. cf. ibid.
4. His full name was Carl Friedrich Wilhelm Ludwig.
5. Burdon-Sanderson, J.—Carl Ludwig, Proceedings of the Royal Society of London, 59: part 2, page I, 1895-96.
6. Lombard, Warren P.—The life and work of Carl Ludwig, Science, p. 363, Sept. 15, 1916.
7. cf. ibid.
8. cf. Burdon-Sanderson, p. I.
9. (a) Faber, Knud—Nosography, The Evolution of Clinical Medicine in Modern Times, 2nd ed., pp. 62-63, N. Y., 1930.
   (b) Haeser, Heinrich—Geschichte d. Medicin und d. epidemischen Krankheiten, 3 Auflage, Bd. II, p. 806, Jena, 1881.
   (c) Garrison, Fielding H.—Introduction to History of Medicine, 3rd ed., p. 425, Phila., 1922.
10. See above (3).
11. Müller, Johannes—Handbuch der Physiologie des Menschen, Bd. I, Coblenz, 1834.
12. Kronecker, Hugo—Carl Friedrich Wilhelm Ludwig. Berliner klin. Wochenschrift, XXV: 466, 1895.
13. cf. ibid.
14. Burdon-Sanderson, p. vii.
15. Müller's Archiv, 1847.
16. Burdon-Sanderson, p. ii.
17. Kronecker, p. 466.
18. Burdon-Sanderson, p. vii.
19. Zwei Grosse Naturforscher des 19 Jahrhunderts. Ein Briefwechel zwischen E. du Bois-Reymond und Karl Ludwig. Herausgegeben von Estelle du Bois-Reymond. Vorwort Anmerkungen u. Register von Paul Diepgen., p. 58, Leipzig, 1927; Ludwig an du Bois-Reymond.
20. cf. ibid., p. 80.
21. Burdon-Sanderson, p. iv; Zwei grosse naturforscher, p. 96.
22. ibid., p. 110.
23. Lombard, p. 365; Ludwig, Carl—Physiologie des Menschen, Bd. I, Leipzig, 1852.
24. Setschenow—Beiträge zur Pneumatologie des Blutes, Wiener Berichte, vol. 36, p. 304, 1859.
25. Kronecker, p. 467.
26. Lombard, p. 366.
27. ibid., p. 368.
28. ibid., p. 374.
29. Cushing, Harvey—Life of Sir William Osler, vol. I, p. 218, Oxford, 1925.
30. Burdon-Sanderson, p. V; Lombard, p. 367, 369.

31. Sabin, Florence R.—Franklin P. Mall: A Review of his scientific Achievement, Science, n. s., 47: 260, 1918.

American Students

Henry P. Bowditch

1. Welch, W. H.—Evolution of Modern Scientific Laboratories Reports of the Smithsonian Institution, p. 498, Washington, 1895.
2. Cannon, W. B.—Biographical Memoir. Henry P. Bowditch. National Academy of Science, 17: 183-194, 1922.
3. cf. ibid.; All biographical details are based upon Cannon's memoir and the article in the Dict. of American Biography.
4. cf. ibid.
5. cf. ibid., p. 184.
6. cf. ibid. Graduating thesis for Harvard Medical School was a review of observations on the physiological action of potassium bromide, some were personal. The thesis was published at the request of Prof. E. H. Clark as a "valuable addition to our knowledge."
7. cf. ibid.—In a letter written after 5 months in Paris, Bowditch says: "I have been devoting myself lately almost entirely to the purely scientific part of my profession which certainly has a much greater attraction for me than the more practical portion."

7A. Willy Kühne (1837-1900) pupil of Claude Bernard, professor of physiology at Amsterdam and Heidelberg. (Garrison, p. 578).

8. Cannon, p. 185.
9. cf. ibid.
10. cf. Packard, F. R.—History of Medicine in the U. S. 1931. v. 2.
11. See description of medical school when W. H. Welch was a student.
12. Cannon, p. 186.
13. Garrison, p. 578.
14. Cannon, p. 186.
15. cf. ibid., pp. 186-7.
16. see above. Introduction.
17. Osler, William—Can. J. Med. Sc., 2: 274, 1877; Cushing, H.. v. 1. p. 153.
18. cf. Cushing, p. 210.
19. Cannon, p. 186.
20. cf. ibid., p. 187.
21. cf. ibid.
22. cf. ibid., pp. 187-88.
23. Bowditch used curare—cf. ibid., p. 188; cf. Garrison, p. 578.
24. see above (17).

25. cf. Packard, F. R.—History of Medicine in the United States, New York, 1931; cf. Cannon, p. 192.
26. cf. Cannon, p. 187.

Charles S. Minot

1. Lewis, Frederick T.—Charles Sedgwick Minot, Anatomical Record, 10: 133-155, 1916.
2. Jonathan Edwards was an ancestor.
3. cf. Lewis, p. 138.
4. cf. ibid., p. 141.
5. cf. ibid., p. 142.
6. cf. ibid., p. 143.
7. cf. ibid., p. 144.
8. cf. ibid., p. 149.
9. cf. ibid., p. 146.
10. cf. ibid., p. 149.
11. cf. ibid.
12. cf. ibid., p. 152.
13. Minot, C. S.—Human Embryology, 1892.
14. cf. Garrison, p. 569.

William H. Welch

1. Flexner, S.—William Henry Welch. A Biographical Sketch. Science, n. s., 52: 417-418, Nov. 5, 1920; MacCallum, W. G.—William Henry Welch 1850-1934. Arch. Path., 17: 1829-32, June, 1934.
2. cf. Flexner, p. 419.
3. For a history of the College of Physicians and Surgeons, see Packard, vol. 2.
4. cf. Flexner, p. 419.
5. cf. ibid., p. 420.
6. cf. ibid., p. 421.
7. Kronecker, Hugo (1839-1914), pupil of Helmholtz, Wundt, Kühne, Traube, professor at Bern (1885-1914). (Garrison, pp. 576-77.)
8. cf. Flexner, p. 422.
9. Cohnheim, Julius (1839-1884), professor of pathology at Kiel (1868-72), Breslau (1872-78), and Leipzig (1878-84). (Garrison, pp. 616-617.)
10. cf. Flexner, p. 422.
11. cf. ibid., p. 423.
12. cf. ibid., p. 424.
13. cf. ibid., p. 426.
14. A feeling of rivalry ran high among the schools at that time.
15. cf. Flexner, p. 429.

16. cf. MacCallum, pp. 829-32.
17. cf. Flexner, p. 430.
18. Flexner, S.—Dr. Franklin P. Mall: An Appreciation. Science, n. s., 47: 251, 1918.
19. cf. MacCallum, pp. 829-32.
20. cf. ibid.
21. cf. ibid.

Franklin P. Mall

1. A. K.—Organization of Embryological Research in America Nature, 96: 625, Feb. 3, 1916.
2. Davenport, C. B.—Franklin Paine Mall. Dictionary of American Biography, vol. XII.
3. Sabin, F. R.—Franklin P. Mall: A Review of his Scientific Achievement. Science, n. s., 47: 254, 1918.
4. Bulletin of the Johns Hopkins Hospital, 56: 140, March, 1935.
5. cf. Sabin, p. 255.
6. Franklin P. Mall (abstract of an Address by W. H. Welch). Johns Hopkins Hospital Bulletin, 29: 111, May, 1918.
7. cf. Sabin, p. 255.
8. cf. ibid.
9. cf. Johns Hopkins Hospital Bulletin, p. 111, May, 1918.
10. cf. Davenport; see (2) above.
11. cf. Flexner, on Mall, p. 251.
12. Barker, L. F., Johns Hopkins Hospital Bulletin, 29: 112, 1918.
13. cf. Barker, p. 112.
14. Stein, Gertrude—Autobiography of Alice B. Toklas, pp. 99-100, New York, 1933.
15. cf. Nature, 96: 625.
16. cf. Flexner, p. 251.
17. cf. Sabin, p. 255.
18. cf. ibid.
19. cf. Davenport; see (2) above.
20. cf. Sabin, pp. 257-258.
21. cf. Davenport.
22. cf. Sabin, p. 258.
23. cf. ibid.
24. cf. Davenport.

Haller

ALBRECHT von HALLER (1708-1777)
(From an oil-painting by Freudenberger)

Fig. 1.

TEXTS AND DOCUMENTS

A DISSERTATION ON THE SENSIBLE AND IRRITABLE PARTS OF ANIMALS [1]

BY

ALBRECHT VON HALLER

London, J. Nourse, 1755

INTRODUCTION BY OWSEI TEMKIN

On April 22nd and May 6th 1752 Albrecht von Haller read two papers before the Royal Society of Sciences of Goettingen. They were published in the following year under the title: De partibus corporis humani sensibilibus et irritabilibus.[2] This work, whose author was then forty-five years old and at the height of his fame as a professor at the newly founded University of Goettingen, is a landmark in the history of physiology in general and of muscular physiology in particular. But unlike other great scientific contributions it does not contain an entirely new discovery. For the phenomena of sensibility and irritability were known long before Haller and the very terms had been coined by others. The significance of Haller's contribution lies in the method by which he approached the subject.[3]

In antiquity Galen had already formulated the doctrine that all sensibility was bound to the nervous system. But since he lacked modern histological methods, his ideas about the distribution of the nervous fibres in the tissues were rather speculative. It was, therefore, a task left to later physiologists to find out which tissues actually were sensible and what was their relation to the nerves.

Galen had moreover maintained that the skeletal muscles were brought into action by the nerves which by means of the psychic pneuma effected voluntary motion through them. The contractile

[1] Reprints of this paper are available through the Johns Hopkins Press.

[2] Commentarii Societatis Regiae Scientiarum Gottingensis T. II, pp. 114-158.

[3] For a general appreciation of Haller's work and its cultural background cf. Stephen D'Irsay, Albrecht von Haller. Eine Studie zur Geistesgeschichte der Aufklärung. Leipzig 1930.

fibres of such organs as the stomach, uterus, intestines and even the heart itself on the other hand were not considered muscular by Galen. They were directed by "natural faculties" and were sharply distinguished from the "psychic" organs: nerves and skeletal muscles.

The Galenic system had, therefore, little room for such a principle as irritability, although there are some statements in Galen and other writers which are dimly suggestive of it.[4] Irritability as a general property was not recognized before Glisson (1597-1677) to whom the term itself is due. Glisson attributed irritability to all fibres but he thought that this faculty presupposed the existence of a "natural perception" of the stimulus in the body.

The history of irritability from Glisson to Haller has been related by the latter himself on pp. 692 ff. of the following reprint and therefore need not be repeated here. Most of the investigators tried to explain the phenomenon either by mechanical or, on the other hand, by vitalistic ideas. It was especially the followers of Stahl (1660-1734) who thought life dependent on the regulation of all physiological processes by the soul. In contrast to all such theories Haller gave to irritability and sensibility a purely experimental definition. He called all parts irritable where a contraction could be observed and, accordingly, he defined all those parts as sensible whose stimulation was either consciously noticed, if the experiment was conducted upon a human being, or caused signs of unrest in animals (cf. pp. 658-659).

On this basis Haller submitted all parts of the body to a long

[4] In book I, cap. 8 of his "De motu musculorum," Galen concedes a natural contractility to the skeletal muscles. For translation and discussion of that passage cf. J. F. Fulton, Muscular Contraction and the Reflex Control of Movement. Baltimore, 1926, p. 5. In book 5, cap. 3 of "De usu partium" Galen, discussing the transmission of foodstuffs from the intestine into the liver and the action of the bile, says (ed. Helmreich, Leipzig 1907, pp. 256, 9-13): "The activity increases in vehemence, when the bile has not yet mingled with the residues [i. e. of the foodstuffs], but still flows around unmingled in the very tunics of the intestines, irritating [verbally: biting] the latter and exciting (ἐρεθιζούσης) them into excretion." Commenting on the Greek term ἐρεθισμός, Foesius (1528-1591) in his Oeconomia Hippocratis, writes: "Ἐρεθισμὸς signifies some provocation (irritamentum) of nature, by which nature being stirred up, is excited into the performance of movements. . . ." These examples whose number could be increased from various ancient authors show that the ground for Glisson's conception had already been broken.

series of experiments performed by himself and his pupils, especially the famous Johann Georg Zimmermann (1728-1795). As a result he arrived at the conclusion that only those parts which are supplied with nerves possess sensibility, whereas irritability is a property of the muscular fibres (p. 690). Thus sensibility and irritability were clearly distinguished and were considered independent of each other.

As soon as Haller's results became known opposition arose on various sides. Many physicians and surgeons claimed that their clinical observations refuted Haller's statements about the insensibility of certain parts, such as the periosteum and tendons. It was from such considerations that De Haën (1704-1776), the famous Viennese clinician, became a violent adversary of Haller. But the more important opposition arose from men like Robert Whytt (1714-1766), Le Cat (1700-1768) and Delius (1720-1791). It was especially Whytt with whom Haller became involved in a long dispute. Whytt was one of the few men who based their contrary views on experiments and he was right in opposing the radical distinction between sensibility and irritability which left no room for such phenomena as reflex actions.[5] But Haller put him on the same level as Le Cat and Delius, adherents of Stahl's animism. Whereas these blamed Haller for his complete separation of the soul from the phenomenon of irritability, the French physician and materialistic philosopher De La Mettrie (1709-1751), writing under the pseudonym of Demetrius, had in his famous work, L'homme machine, used irritability as an argument against the very existence of a spiritual soul. Haller, himself a very pious man, was highly indignant at De La Mettrie who in his writings moreover had ridiculed him and made him the laughing stock of all Europe.

All these controversies are reflected in Haller's essay in which he again and again insists on the experimental character of his work. But physiological theories, however experimental the method of their approach may be, have always a bearing on philosophical problems and Haller could not evade a discussion of the metaphysical consequences of his work.

In 1753, the same year in which the present treatise was first published, Haller left Goettingen to return to his native Switzerland

[5] This has been brought out very clearly by John Fulton, l. c. pp. 32-34.

where he died in 1777. It was during this period that he published his famous Elementa Physiologiae in eight volumes, and here and on many other occasions he returned to the problem of sensibility and irritability. Even as late as 1772 and 1773 he submitted a third and fourth communication on the subject to the Royal Society of Goettingen, following his first papers of twenty years before.

Like many others of Haller's views, those on sensibility and irritability were not destined to outlive him in every respect. Later research proved that his analysis of the sensibility of various tissues had not been correct. And concerning irritability, his definition was certainly too limited. He had identified it with contractility and confined it to the muscular fibres, whereas at the end of the eighteenth century irritability was recognized as a general vital principle extending over all tissues. It was especially John Brown (1735-1788) who built a whole medical system on the conception of irritability, or "excitability" as he called it. According to Brown "The common effect, produced by the exciting powers, is sense, motion, mental action, and the passions."[6] And he added: "We know not what excitability is, or in what manner it is affected by the exciting powers. But, whatever it be, either a certain quantity, or a certain energy of it, is assigned to every being upon the commencement of its living state."[7] Proceeding from such general statements, Brown tried to explain pathological processes and their therapy as dependent on the power of the stimulus and the degree of excitability. Xavier Bichat (1771-1802), on the other hand, considered sensibility and contractility the basic characteristics of life: "Sensibilité et contractilité, voilà les propriétés vitales."[8] He distinguished between their organic and animal stages, and identified irritability with "sensible organic contractility" which first appeared in lower animals, plants possessing "insensible contractility" only.[9] The Brownian system was overcome during the first half of the nineteenth century and Bichat's work is now one of the historical steps in the development of medical sciences. Yet irritability or excitability have remained fundamental physiological

[6] John Brown, The Elements of Medicine. Vol. I, London 1788, p. 5.
[7] Ibid. p. 7.
[8] Xav. Bichat, Anatomie générale. T. I. Paris 1801, p. xxxv.
[9] Cf. ibid. pp. xli-xlii.

conceptions, although their nature is still discussed as it was at Haller's time. But apart from the stimulating effect Haller's treatise exercised on the physiologists of his epoch, it is an outstanding document of indefatigable experimentation and clear reasoning. As such it has become a classic.

In 1755 the Swiss physician and Haller's friend, Simon André Tissot (1728-1797), published a French translation of the emended Haller text [10] together with that of another short paper of Haller's on the cause of the movement of the heart. Tissot added moreover a lengthy and enthusiastic preface and Haller himself contributed a short supplement.

The anonymous English translation which appeared in the same year is obviously based on the French edition of Tissot,[11] whose name it even mentions on the title page (cf. fig. 2). It, too, contains Tissot's preface (pp. i-xxxii), Haller's supplement (pp. 67-73) and the Essay on the Cause of the Motion of the Heart (pp. 74-79). The latter, since originally an independent publication, together with Tissot's preface, has been omitted from the present reprint. Haller's supplement, however, as well as the bibliographical references has been included. The original orthography has been preserved, correction having been limited to some obvious misprints and a few minor slips.

[10] Dissertation sur les parties irritables et sensibles des animaux. Par M. de Haller. Trad. du Latin par M. Tissot. Lausanne, M. M. Bosquet et Comp. 1755. According to J. G. Zimmermann, Das Leben des Herrn von Haller, Zürich 1755, p. 296, this translation was brought out at the end of 1754. It had been preceded by translations into Swedish and German.

[11] As shown by comparison with the French and Latin texts.

A

DISSERTATION

ON THE

Senſible and Irritable Parts

OF

ANIMALS.

By M. A. HALLER, M. D.
Preſident of the Royal Society of Sciences at Gottingen: Member of the Royal Academy of Sciences at Paris: *&c.*

Tranſlated from the LATIN.

With a PREFACE by M. TISSOT, M. D.

LONDON,
Printed for J. NOURSE at the *Lamb* oppoſite *Katherine-ſtreet* in the *Strand*.
MDCCLV.

Fig. 2.

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A TREATISE ON THE Sensible and Irritable Parts OF ANIMALS.

SOME months ago[a] my pupil and friend Dr. ZIMMERMAN published an inaugural dissertation upon Irritability; part of the experiments relating to that subject I saw him perform, and I shall relate them here, in the same order as I find them, in my notes. Others of them I was not witness to, and therefore I shall quote these from his thesis. Since the year 1746, I have made a great many of the same kind myself, which he was either a witness of, or assisted me in performing; and since the beginning of the year 1751, I have examined several different ways, a hundred and ninety animals, a species of cruelty for which I felt such a reluctance, as could only be overcome by the desire of contributing to the benefit of mankind, and excused by that motive which induces persons of the most humane temper, to eat every day the flesh of harmless animals without any scruple. As in making these experiments I was obliged to try several which were useless, and to repeat others of them several times, to communicate the whole of them would only be spinning out this treatise needlessly; wherefore I shall confine myself to relate those only which are of real use, and are found constantly true.

The result of all these experiments has given place to a new division of the parts of the human body, which I shall follow in this short essay, by distinguishing those which are susceptible of Irrita-

[a] Dr. HALLER read this memoir in the academy of Goettingen, April 22, 1752, and the dissertation which he quotes was published in July 1751, under the following title, *Dissertatio Physiologica de Irritabilitate, authore* JOHANNE GEORGIO ZIMMERMAN Helveto Brugensi.

bility and Sensibility, from those which are not. But the theory, why some parts of the human body are endowed with these properties, while others are not, I shall not at all meddle with. For I am persuaded that the source of both lies concealed beyond the reach of the knife and microscope, beyond which I do not chuse to hazard many conjectures, as I have no desire of teaching what I am ignorant of myself. For the vanity of attempting to guide others in paths where we find ourselves in the dark, shews, in my humble opinion, the last degree of arrogance and ignorance.

I the more willingly engaged in examining this subject, that the experiments which I have made, are the source of a great many changes, both in the physiology, pathology and surgery, and discover several truths contrary to the opinions generally received. This last reason has obliged me to be very exact in my proofs, because I was fully persuaded, that an opinion so little foreseen would at first appear improbable, and only gain assent by the clearest conviction. For this reason I was obliged to repeat and multiply my experiments, in order to convince the incredulous, by a number of authentic testimonies, so to speak, and prevent my falling accidentally into any mistake; for I am persuaded that the great source of error in physic has been owing to physicians, at least a great part of them, making few or no experiments, and substituting analogy instead of them. A second motive which encouraged me in this work, was the readiness with which some celebrated authors have laid hold of the first notions of Irritability, so as even to make use of this property of our fibres, as a basis of almost an universal system of motion in the human body, and thence deduce the functions of the fibres, vessels, nerves, muscles, and in short of all our organs. This appears very plain in reading Dr. WINTER's oration, delivered at Franeker in 1746, that of Dr. LUP's *De Irritabilitate*, and the thesis of DE MAGNI and LA MOTTE upon the following subject, *Ergo a vasorum aucta aut diminuta irritabilitate omnis morbus*. For they all agree pretty much in the same opinion, *viz.* in deriving all motion from sensation: In which they are joined by Drs. KRUGER, NICOLAÏ, WHYTT, DELIUS, and some other great physiologists.

I call that part of the human body irritable, which becomes shorter upon being touched; very irritable if it contracts upon a slight touch, and the contrary if by a violent touch it contracts but little.

I call that a sensible part of the human body, which upon being touched transmits the impression of it to the soul; and in brutes, in whom the existence of a soul is not so clear, I call those parts sensible, the Irritation of which occasions evident signs of pain and disquiet in the animal. On the contrary, I call that insensible, which being burnt, tore, pricked, or cut till it is quite destroyed, occasions no sign of pain nor convulsion, nor any sort of change in the situation of the body. For it is very well known, that an animal, when it is in pain, endeavours to remove the part that suffers from the cause that hurts it; pulls back the leg if it is hurt, shakes the skin if it is pricked, and gives other evident signs by which we know that it suffers.

We see that experiments only can enable us to define what parts of the human body are sensible or irritable, and what the physiologists and physicians have said upon these qualities, without having made experiments, has been the source of a great many errors, both in this case and in a number of others.

When Dr. BOERHAAVE had established the doctrine of the nerves, being the basis of all our solids, he presently after proceeded to affirm, that there was no part of the human body which was not sensible, or capable of some sort of motion,[b] and this system, which I have elsewhere [c] refuted, was received almost universally all over Europe.

The most simple parts of the human body are the nerves, arteries, veins, the smaller vessels, membranes, the muscular, tendinous, ligamentous, and bony fibres, and the cellular membrane.

The more compound parts are the muscles, tendons, ligaments, bowels, glands, the great reservoirs, excretory ducts, and the larger blood vessels.

But this by the by, for here I don't study accuracy, this being only a general list of the parts of the human body.

In order to discover which of these parts are sensible, I made the following experiments.

I took living animals of different kinds, and different ages, and after laying bare that part which I wanted to examine, I waited till

[b] Institut. Med. N° 301.

[c] *Commentar. in* Prælect. Boerh. loc. cit.

the animal ceased to struggle or complain; after which I irritated the part, by blowing, heat, spirit of wine, the scalpel, *lapis infinalis*, oil of vitriol, and butter of antimony. I examined attentively, whether upon touching, cutting, burning, or lacerating the part, the animal seemed disquieted, made a noise, struggled, or pulled back the wounded limb, if the part was convulsed, or if nothing of all this happened. The repeated events of those experiments I marked down faithfully, whatever I found them to be. For what is it to me, in fact, on which side nature decides the question? nay, would it not be very foolish to hazard the reputation of a faithful and accurate observer for an imaginary fact, which the simplest experiment would prove to be false to any other anatomist who should chuse to repeat it?

As to the order of making the experiments, I think it is of no great consequence, and therefore shall begin with the *skin*. For as to the epidermis, that is certainly insensible, seeing you may burn it with hot spirit of nitre till you give it a durable yellow taint, without occasioning the least degree of pain.

The *rete mucosum* of MALPIGHIUS can hardly be separated from the epidermis in experiments of this kind; wherefore I did not try any upon it, being fully convinced that it has no feeling.

The skin is sensible, and indeed more so than any other part of the body; for in whatever manner you irritate it, the animal makes a noise, struggles, and gives all the marks of pain that it is capable of. This great sensibility of the skin has determined me to take it as a fixed standard of that property; and I reckon as insensible, or at least only in a small degree, those parts which may be irritated without disturbing the animal, while it discovers signs of pain if the neighbouring skin is irritated.

The fat and cellular membrane are not sensible of pain, as is very well known, and has been shewn by other authors. What is told of DIONYSIUS the tyrant, and is commonly known to hold true in hogs, may be sufficient to confirm this; *viz.* upon thrusting a needle into them they shew no signs of pain, till it has got quite through the fat and reached the flesh below.[d]

[d] Commentar. BOERH. tom. III. N° 333. not. [b]

The *muscular flesh* is sensible of pain, but that is owing more to the nerves than to the flesh itself. For if you tie the nerve of any joint, if there is only one, or the principal trunks, in case there are more, the whole joint is thereby rendered insensible, nor is the animal affected by those applications which hurt the joint when the nerves are tied. For it is very well known that all the muscles are capable of pain, the large hollow ones not excepted, such as the stomach, intestines, and bladder.

But the *tendons* are neither capable of sensation, nor pain. This is the first paradox which I advance against the common opinion, and it has hitherto found but few partizans. Almost all the antient, and some of the most modern authors, as La Faye,[e] Heister,[f] and Garengeot,[g] look upon wounds of the tendons as very dangerous and difficult of cure, and the same opinion has been adopted by Boerhaave, his worthy pupil Van Swieten,[h] by Acrell[i] and Quesnay.[k]

I shall shew immediately, that this opinion was not altogether unknown before. For Job a Mekren,[l] a very experienced surgeon, has said that the tendons are very insensible, and he quotes as an instance that of the *patella*. Bryan Robinson[m] asserts, that in a living dog irritations of the tendons did not seem to be very painful, but the muscles were more sensible. George Thomson observed[n] no commotion from a hurt of a tendon, and Daniel Schlichting[o] was witness of the same thing both in men and in dogs. But these authors are not numerous, and made only few experiments.

For the most part I have laid bare the tendon of the *extensores recti* of the *tibia*, or the *tendo achillis*, after which I have pricked it, cut off part of the fibres, even to one half of them, nay I have cut the tendon across so as to leave one half of it intire, which wound Boerhaave looked upon as the most dangerous. These experiments I have repeated upwards of a hundred times since the year 1746,

[e] Chirurgie de Dionis derniere edition, pag. 680, 681.
[f] Inst. Chir. pag. 423, edit. de 1737.
[g] Operat. de chir. tom. III. ch. 7.
[h] Tom. I. n. 163. p. 238.
[i] Om Friskasor pag. 261. seqq.
[k] De la supurat. pag. 222.
[l] Obs. cent. pag. 162.
[m] Animal œconomy p. 90.
[n] Anatomy of human bones p. 170.
[o] Traumatograph. p. 213. Eph. nat. cur. vol. VI. obs. 24.

upon dogs, goats, rats, cats, rabbits, and various other animals, always with the same success.

The use of this experiment is to prove, that if you irritate the muscular fibres they contract; but it is not the same case with the tendon, which you may prick and lacerate as much as you please, without occasioning the least motion of the muscle; as indeed in general, when the muscle is contracted, there is never any contraction observed in the tendon, which WILLIS [p] has taken notice of before, and I have seen it a hundred times. Hence therefore it appears, that the tendons have no organ neither of motion nor sensation.

The constant event of this experiment was, that the animal, whose tendon was lacerated, burnt or pricked, remained quiet, without shewing any sign of pain, and when it was let go, provided only a small part of the tendon remained whole, it walked easily and without any complaint. I have seen a dog walk upon his two hind feet, when the *tendo achillis* of both legs was pierced through the middle, and a kid walk freely, after I had cut away one half of each of the same tendons. And in another dog, who had only the tendon of the *solearis* muscle intire, those of the *gastrocnemii* being cut through and drawn up into a kind of knot; I observed no bad symptom as long as I kept him. Wounds of the tendons are likewise the most easily cured of all, only by the help of nature, without any trouble or bad symptoms, so that there is nothing surprising in that observation of M. la FAYE,[q] *viz.* of the tendon of the biceps being cut without occasioning any want of motion in the arm. Neither ought we to blame VESLINGIUS and some others [r] for boldly recommending the sutor of the tendons; nor M. BIENASE [s] for having ventured upon it, after having first tried the experiment on a dog. Dr. ZIMMERMAN [t] observed no sensation in the *aponeurosis* of the *abdomen*, upon touching it with oil of vitriol.

After I was fully satisfied of the certainty of the above event, I easily discovered the cause, *viz.* that there are nerves distributed to

[p] De motu muscul. p. 118. compare BAGLIVI opera p. 317.
[q] Chir. de DIONIS pag. 681. note [a].
[r] Epistol. posth. a BARTHOLINO.
[s] VERDUC Operat. de Chirurg. c. 32. [t] Dissert. cit. p. 16.

the muscles, but not to the tendons. FABRICIUS [u] AB AQUAPENDENTE owned long ago, that he could not trace the nerves to the tendons, but they disappeared in a kind of membrane before they got that length; and LEUWENHOEK [x] could discover only a very few nervous filaments upon the surface of the tendons.

Seeing therefore in the human body the nerves only are capable of sensation, it is neither unnatural nor improbable, that the tendons being destitute of nerves should have no sensation. I have oftener than once seen the tendons laid bare in man, and, emboldened by the experiments which I made upon brutes, I once laid hold, with a pair of *forceps*, of the naked flexor of the third joint of the fore finger, of a young gentleman, without his being sensible in the least that I touched it. I have likewise seen the tendon of the long *supinator* muscle chafed with hot oil of turpentine, in order to stop an hæmorrhage; it occasioned an acute pain in the skin, but the patient felt none in the tendon. Thus it is an old practice to make use of hot oil of turpentine in wounds of the tendons, and surgeons have long ago looked upon it as an excellent remedy in such cases; but this oil would certainly occasion as much pain in the tendons, as it does in the skin, provided they were sensible.

Wherefore we need no longer be afraid of wounds of the tendons, of whatever kind they are. If a large tendon is destroyed, it is evident the patient must halt, or lose the use of a joint, seeing the muscles, being thereby deprived of their insertions into the bones, can act no longer upon it. But besides this want of motion, there is nothing else to be feared, and sometimes nature remedies this misfortune in such a manner, by means of a new cellular membrane, and the neighbouring muscles, that the motion of the joint is performed as easily as before. I have seen a new cellular substance of a bluish colour, grow in a few days, and unite the ends of the *tendo achillis* that was cut through in a dog. As soon as they were united the animal suffered no more inconvenience, and jumped with the same agility as before upon the chairs and tables.

Whence therefore can this mistake arise with regard to wounds of the tendons, into which almost all the authors have fallen, who

[u] De Fabric. muscul. pag. 27.

[x] Epist. Physiolog. pag. 443.

have wrote upon the subject, even the most respectable and intelligent of them in other respects? It appears to me to be most probably owing to the signification of the word νεῦρον, being confounded with those of τένων and σύνδεσμος, whereby it is made to signify both *nerve, tendon,* and *ligament.*[y] But a wound in a nerve produces most violent symptoms, as will immediately appear. Thus I really believe, that in bleeding, a wound of the *median* nerve, and perhaps sometimes of a branch of the *musculocutaneous,* which runs down before the median vein, has produced those painful symptoms, which are commonly imputed to pricking the tendon of the biceps muscle, over which the vein runs. PARE' has left us an account of a remarkable instance of this in CHARLES IX. of France. It is likewise to the great nerves which are distributed the whole length of the fingers, that we ought to attribute the causes of the unlucky events of some kinds of whitlow, the danger of which has long been ascribed to their seat being in the sheaths of the tendons, as was the opinion of GARENGEOT[z] not long ago.

The *ligaments* and *capsulæ* of the articulation, are situated in the neighbourhood of the tendons; the former have been described under the name of νεῦρον, and the latter are famous on account of the danger alledged to attend wounds of them, and of their being blamed by many celebrated physicians as the seat of the gout.[a]

In making these experiments I met with a good deal of difficulty. For as it was necessary to remove the skin, and to stretch it a good deal in the tight joints of the smaller kinds of animals, so as to allow the wounding and irritating instrument to be introduced into the hollow of the articulation, the animal frequently complained, upon my laying hold of the skin with the *forceps.* However the experiment, even when it was made with poisons, very often succeeded. In a cat I filled the articulation of the *femur* with the *pelvis* with oil of vitriol, without its seeming to feel this violent corrosive, although I have seen the uterus of a bitch destroyed in the space of a minute by the same application. In making these experiments

[y] GALEN de usu part. L. XV.

[z] Operat. de Chirurgie. tom. III. pag. 286, 301, 302.

[a] BOERHAAVE *aphoris. de cognosc. & curand. morb.* 1254, 1259. It is true this great man admitts likewise the nerves to be the seat of this disease.

upon the articulation of the knee, which is more readily come at because it is almost bare, I have frequently made use of small pieces of sticks dipt in oil of vitriol, or in the butter of antimony, with which I have burnt the lateral ligaments, that of the *rotula*, both the external and the internal surface of the *capsula*, and HAVERS's gland, without the animal discovering the least sense of pain. Nay these wounds, which are commonly reckoned the worst, were cured with so much ease, that the saliva alone of the animal was sufficient for it, and very often they healed without it. Now all these experiments, which have been repeated upon dogs, cats, and young kids, justify the observation of M. DE LA MOTTE,[b] who found the *extensor* ligament of the *tibia* to be insensible. Sometimes instead of caustics, I tried the experiment with a knife or needle thus: I made an incision on the external side of the articulation of the knee, laid bare the *capsula*, *patella*, the ligament which goes from it to the *tibia*, and the external or internal lateral ligament; then scraping with a knife the external surface of the *capsula* and ligament, I thrust a knife or needle thorough from the inside till its point pierced the skin, during all this the animal shewed no signs of pain, till the point of the knife or needle, after having pierced the *capsula* of the articulation, came to touch the cellular membrane under the skin. This experiment I have repeated several times.

Hence it appears, that we ought not to attribute the sharp pains of the gout to the *capsula* of the joint, in which it is so difficult to find any nerves, and which is void of sensibility: Their true seat is in the skin, and in the nerves which creep upon its internal surface. And it was certainly very well ordered by nature, that those parts which are exposed to a continual friction should be void of sensation.

Wherefore if wounds of the joints are sometimes difficult to heal, we must attribute this to the humour which is continually secreted there, and which readily acquiring a rancid putrefaction, will not allow the wound to heal up. In dogs, as far as I can remember, these wounds always healed without any difficulty.

The *periosteum* resembles the ligaments and *capsulæ*, and evidently in the *fœtus* forms together with them a thick pulpy mem-

[b] Chirurg. compl. N° 365.

brane, which is continued from one bone to another, including the intermediate articulation. Wherefore I was not surprised to find it insensible in the numerous experiments which I made in the leg, thigh, heel, *metatarsus*, and *pericranium*, which last is of the same nature with the *periosteum*.

Physicians, anatomists,[c] and surgeons, who are of another opinion, will I hope pardon me for differing so widely from them, and defer condemning me, till they have compared the experiments upon which that opinion is founded. A hundred times have I tore, pricked, and burnt the periosteum, without the animals shewing the least sense of pain; nay young kids have sucked during the time, while if I pinched the skin they complained violently, and fell into convulsions.

This insensibility of the periosteum was observed before by Mr. CHESELDEN, and it is not at all surprising in a part where there are no nerves, where even NESBIT[d] himself could not discover any, but concluded there must be such, though they were invisible, to account for the sensibility which he ascribed to this membrane. For those which creep in great abundance upon the *pericranium*, and do not come from the tenth pair, but from the second and third of the neck, and from the third and fifth of the brain, are bestowed upon the skin of the head, and communicate their sensibility to it.

The insensibility of the *bones* is still disputed, and indeed I have made no experiments upon this subject, for in that cruel torture which is unavoidable in order to lay bare the bones, it is very difficult to distinguish the new pains thereby produced. The sensibility of the *teeth* is known to everybody, but the same reason which explains that, persuades me that the bones are void of sensation. For though the nerves of the teeth are extremely slender, yet they are easily shewn where they enter their proper foramen. But in the large bones I have never been able to find any nerve, accompanying the artery and vein at their entry into the bone;[e] if there were, I

[c] WINSLOW, Traité des os frais N° 60. CLOPTON, HAVERS, NESBIT *human osteogen.* p. 6. Phil. Ad. BOEHMER. osteol. p. 31. DUVERNEY Traité des maladies des os, tom. II. p. 431.

[d] Loc. cit. ut supra.

[e] Nervi ad ossa nulli, RIOLAN enchirid. Anatomic. AL. MONRO loc. cit. p. 16.

should have discovered them in my numerous descriptions of the arteries; if not elsewhere, at least in the large and smooth internal surface of the cranium, and in my preparations of the nourishing arteries of the whole body. DIDIER [f] says that the bones rendered soft are very sensible; but besides that it is easy to be imposed upon in so terrible a disease, M. IMBERT [g] asserts the contrary; and I have seen the operation of the trepan performed upon sound persons who had the free use of their senses, without their complaining of any pain from the perforation of the *cranium*.

DEVENTER,[h] AMBROSE PARE,[i] DU VERNEY,[k] and most other authors, agree in saying that the *marrow* is very sensible of pain; but this is very improbable, as it is of the same nature with fat, and has no nerves bestowed upon it.

The *dura mater* is a kind of *periosteum*, every where applied to the internal surface of the *cranium*, connected to it by vessels, and has arteries transmitted through it to the skull, in the same manner as they are transmitted through the periosteum to the other bones of the body. PACCHIONI and BAGLIVI have ascribed to it a force equal to that of the heart, and the most part of physicians have looked upon it as the seat of the most violent diseases; but their opinions do not change the eternal nature of things. I have shewn elsewhere,[l] that it was, like all the other membranes of the body, composed of the *membrana cellularis*, and this analogy has been confirmed by the experiments of Messieurs ZINN,[m] ZIMMERMAN,[n] and CASTEL, and likewise by my own, which convince me that this membrane, so nearly resembling all the others produced from it, may be burnt with oil of vitriol, butter of antimony, or spirit of nitre, cut with a knife, or tore with a pair of pincers, without the animals appearing to suffer the least pain. Messieurs ZINN and MEKEL observed the same insensibility in the *dura mater* of a man, which had been laid bare by a *caries* of the *cranium*; and doubtless the antient physicians, CARDANUS,[o] and before him GALEN, spoke

[f] Anat. rais. p. 6, 7.
[g] Quæst. med. XII. p. 33.
[h] Van beensiekten p. 80.
[i] Administr. anat. p. 83.
[k] Mem. de l'acad. des scienc. 1700. pag. 205.
[l] Prim. lin. physiolog. n. XI.
[m] Experim. circa corpus callosum cerebellum *&c.* Gotting. 1749 p. 28. seqq.
[n] Loc. cit. p. 6. *&c.*
[o] De vulner. cap. p. 139.

from experience, when they recommended the sharpest applications as not only safe, but requisite in treating the *dura mater*. Comparative anatomy likewise informs us, that this membrane is a true cover for the brain, and not a muscle; for in the tortoise it is found of a cartilaginous consistence.[p]

How is it possible, that a membrane so insensible and incapable of motion, should have the force to send back the spirits to the heart, and be the seat of head achs, the *phrenitis* or madness, unless you chuse to alledge, that when it is affected, the brain by its proximity ought to sympathise? wherefore the French surgeons are very right in venturing to cut it, as often as they observe *pus* or extravasated blood under it.

I must here beg leave to make a digression, which perhaps may be of use. M. SCHLICHTING [q] has said, that there was a motion in the brain, that it ascended and descended alternately, and is extremely angry with those who rank it amongst those parts of the body which are void of motion. Being fully convinced of the strong adhesion of the *dura mater* to the *cranium*, and of the total plenitude of the contents of the skull, I could not help wondering at the boldness with which this author maintained the contrary; however I did not think of refuting him by authorities or reasons *a priori*, but judged it best to fight him with his own weapons, that is by experiments. Wherefore I opened the skulls of several dogs with a hammer and chissel, which is a more commodious way than with a trepan, and exposed to view a large part of the brain; and this experiment I frequently repeated upon dogs, goats, rats, frogs, cats, and other animals. The event was always the same, *viz.* I saw a manifest motion in the *dura mater*, or rather in the whole brain, such as SCHLICHTING has described, for it ascended in expiration and descended in inspiration. Upon account of this motion alone I made above thirty experiments with M. WALSDORF, who proposes shortly to publish a small treatise upon this subject.[r]

I am too great a lover of truth to suffer pain upon account of a new discovery, however opposite it may be to my former ideas; but

[p] STEPH. LORENZINI obs.

[q] Memoir. présentes tom. I. p. 114. *et seq.*

[r] He published it in 1753, since the first publication of this treatise.

what gave me uneasiness was, that I could not discover the reason of this correspondence between the motion of the brain and respiration: For the mind loses patience at the discovery of a phænomenon which seems repugnant to reason.

But repeated experiments removed this seeming contradiction. Neither the dura mater nor the brain have any motion unless the cranium is removed, which therefore in a living sound animal is an effectual obstacle to it. This SCHLICHTING himself owns to be the case,[s] nay for the most part there appears no motion in the brain, till the adhesion of the dura mater to the bones of the *cranium*, which keeps the brain immoveable, is destroyed by the fingers or some proper instrument. Wherefore this consent of the motion of the brain with respiration can never be applied to a sound healthy person; for if the dura mater is not moved so long as it adheres to the cranium, and if the brain is only raised up in expiration after the dura mater has been separated from the skull, which is never the case of a person in health, this phænomenon ought not to be regarded as really existing.

Farther, I did not find this peculiar to the brain, but from repeated experiments I have observed the same in the larger veins, *viz.* in both the *cavæ*, the fubclavians, the upper part of the basilic, and the jugulars; for all these swell during expiration, and become of a deep blue colour, whereas in inspiration, being more empty, they subside and look pale. The phænomenon therefore which SCHLICHTING has observed is not peculiar to the brain, as I took notice of before, and seems intirely to depend upon that facility with which the blood passes from the right ventricle of the heart into the lungs, which are dilated while we inspire, and upon that with which the *venæ cavæ* empty themselves into the same ventricle in the time of inspiration.[t] On the contrary, in exspiration the lungs being compressed, resist the passage of the blood from the heart, and the blood contained in the heart resists that returning from the extremities; hence the large veins, and amongst these the jugulars swell, and thereby the brain is rendered turgid with blood.[u] I know very well

[s] Ibid. pag. 116.

[t] Primæ Lineæ Physiol. N° 292.

[u] Ibid. § 297.

that by prolonging inspiration voluntarily, we retard the blood which passes through the lungs;[x] but in the ordinary alternate motion of respiration, the blood enters more easily into the lungs during inspiration, although in a preternatural state, when the lungs are filled with blood, and for want of expiration, it cannot pass to the left ventricle, there must thereby be produced a dilatation of the right, and a stagnation in the veins, almost equal to that which naturally accompanies expiration.

Allow me to add in two words, that the longitudinal sinus has no pulsation, even after the *cranium* has been removed, and that when it is pierced, the blood does not fly out in jirks, but runs in a continued stream, as when one opens a vein; and this confirms what I published before against the pulsation of the sinuses of the brain.[x] It is owing to the same cause, that the small arteries which go from the *dura mater* to the *cranium*, and of which the greatest number take their origin from the surface of the large *sinus*, may be filled with injection, without its ever penetrating into the sinus itself.

The followers of STAHL, and others, especially GOHL, who deny the existence of the animal spirits, conceive the nerves to be like tense cords, which are put in motion by the impressions of objects, and communicate their vibrations to the *meninges* of the brain, which they look upon as the organ of all our sensations. This theory I have refuted by several arguments, which I see have not only satisfied Dr. FLEMMING,[y] but likewise the most modern patrons of the doctrine of the soul governing the body, admit the animal spirits, as Dr. WHYTT.

But there is still another argument, which proves more fully, that the faculty of sensation, whatever it is, does not reside in the membranes of the nerves. With regard to the *dura mater* I am fully convinced, that it does not form the external coat of the nerves, although most other anatomists are of a different opinion. But there remains the *pia mater*, which actually sheaths each of the medullary fibres, which are so very slender, that we can count near a hundred of them in one of the branches of the fifth pair. If therefore I can

[x] Ibid. § 294.
[x] Comment. ad inst. BOERH. N° 234.
[y] Of the nature of the nervous fluid, Lond. 1751. 8vo.

shew that the *pia mater* is void of sensation, there can be no reason given, why the sensibility of the nerves, which is owing to the *medulla*, should be ascribed to the membranes. In order to this, I have tried the following experiment upon dogs and kids, and frequently repeated it.

I laid bare the *pia mater*, by removing a part of the skull and the *dura mater* contiguous to it; I touched it with butter of antimony (which ought in this case to be preferred to the oil of vitriol, as this consumes the membranes too fast, and it is almost impossible to prick the *pia mater* with a knife without wounding the brain at the same time) whereupon it was burnt to an *escar*, without the animal's complaining in the least, making any sort of strugling, or being at all convulsed. But upon touching the brain, in whatever manner I did it, the animal was instantly seized with violent convulsions, which bended its body to one side in the form of a bow.

If the *dura* and *pia mater*, and likewise the *periosteum*, are found void of sensation, it appears evidently, that the sensibility of the *other membranes* must be given up. This conjecture is confirmed by repeated experiments, which I have made very carefully upon the *peritonæum*, after removing the *recti* muscles, upon the *pleura* freed from the intercostal muscles and nerves (which is a difficult experiment, but I have frequently done it, and most successfully on a kid, it being a very quiet animal) and even upon the *pericardium*, in all which cases the membranes being cut or irritated, occasioned no sense of pain, nor the least change in the animal. The celebrated STORCH, as appears by the journal of the disease of which he died, was not sensible of any pain, while, in performing the *paracentesis*, the *trocar* pierced the *peritonæum*. Upon this occasion I foresee that many eminent physicians, who place the seat of the violent pain of the pleurisy in the membrane of the *pleura*, will differ from me in opinion. But I only relate facts which I have actually seen.

Neither ought these truths to be looked upon as paradoxes, because they contradict some pathological doctrines. BOERHAAVE[s] affirmed long ago, that in inspiration the *pleura* was less upon the stretch, because by the ribs at that time approaching nearer to one another, the intervals between them were thereby diminished; and

[s] In his *Praxis Medica*, 1745, tom. IV. pag. 162.

that on the contrary, in expiration, that membrane was more tense, because then the ribs receded to a greater distance from each other. But in the pleurisy the patient suffers most in inspiration, that is, when the *pleura* is least distended, and *vice versa*; wherefore that great man did not place the seat of this disease intirely in the *pleura*, but he likewise joined to it an inflammation in the muscles which serve to bring the ribs nearer to one another. But according to my system, in order to explain this phænomenon, it is sufficient that the intercostal nerves suffer, be the case as it will in other respects.

The *mediastinum*, being a very fine membrane, and very much resembling the *omentum*, I consider in the same light with the *pleura*. All these membranes being destitute of nerves, are of the same nature with the cellular membrane, and therefore are not sensible of pain.

The *arteries* and *veins* seem not susceptible of pain; for upon irritating or laying hold of a nerve the animal complains, but if you lay hold of an artery it does not feel it. The sensibility which is occasionally met with in the membranes of the carotid, the lingual, temporal, pharyngal, labial, and thyroidal arteries, and of the *aorta* near the heart, depends upon the nerves which I commonly demonstrate there, and which do not appear to make any farther progress. In those places it is natural to imagine, that the arteries must be sensible, seeing the nerves run immediately upon them, but otherwise they are endowed with little or no sensation. I have frequently ordered the arteries to be tied very tight in men, without their complaining of pain in the least.

The *internal membranes* of the *stomach, intestines, bladder, ureters, vagina* and *womb*, being of the same nature with the skin, ought of consequence to enjoy the same sensibility.

The *heart* is likewise sensible, which I don't say from my own experiments, but from those of other persons; and it is natural to think it should, seeing it is a muscle, and has nerves bestowed upon it. The reason why I have not discovered it myself is, that an animal whose *thorax* is opened is in such violent torture, that it is hard to distinguish the effect of an additional slight irritation.

On the other hand, I am fully convinced, from a great number of experiments, that the *viscera*, properly so called, *viz.* the *lungs*,

liver, spleen and *kidneys,* have very little sensation; seeing I have irritated them, thrust a knife into them, and cut them to pieces, without the animal's seeming to feel any pain. Dr. ZIMMERMAN [a] has tried the same experiments with the like success. Hence ulcers of the lungs are not attended with any pain, and persons have had stones in their kidneys for several years without ever being sensible of them.

If it should be objected that there are nerves in these viscera, my answer is, I do not alledge that they are void of all sensation, but only that it is very weak in them, *viz.* such as one would expect in a part which has very few nerves bestowed on it in proportion to its bulk. For all the *viscera* have large blood vessels and small nerves, the liver not excepted, but especially the spleen and the kidneys.

The *glands* in general have only an obtuse sensation, owing to the few nerves which are bestowed upon them. Hence schirrous and encysted tumours are so indolent, and it is very surprising, that M. DU BORDEU, so severe a critic of the writings of others, should lately have established it as an axiom, that a great many nerves are bestowed upon the glands, and upon this founded a system to explain the nature of their functions, in which he alledges, that it is not compression, but irritation that makes them discharge their liquors. In the mean while it is easy to prove, that there are no considerable nerves bestow'd upon the largest of the glands nor the *thymus,* as far as hitherto we have been able to discover; that the nerves which go to the thyroid gland are a great deal smaller than those of a muscle ten times less in bulk than that gland, and that there is not a gland in the whole body, in which we can demonstrate any nerve of the larger sort. Besides upon opening the mouth, when we are not at all hungry, we can see the *saliva* squeezed out, by the sole compression of the *digastric* muscle.

The breasts are covered with a great deal of skin, and furnished with many nerves. The *pœnis,* which has likewise a great deal of skin, and receives a greater number of nerves than any other part of the body of an equal size, has also a proportionable degree of sensibility. The *tongue,* which has likewise a great many nerves, is endowed with a sensation more acute and delicate than the touch,

[a] Loc. cit. p. 17.

which form the taste. We may judge of the sensibility of the *eye*, and especially of the *retina*, from the irritation and inflammation occasioned by too glaring a light. The *tunica choroidis* and the *iris* seem likewise to be sensible. But I do not find that the *cornea* has any nerves, and hence it may be pierced with a needle without occasioning any pain; and what persuades me that the *iris* is much less sensible than the *retina*, is, that if after having pierced the *cornea*, you irritate or cut the *iris*, it is not thereby contracted, whereas the least increase of light makes it contract; which evidently proves, that this contraction does not depend upon the proper sensibility of the *iris*, but on that of the *retina*. The *gutta serena* serves likewise to prove the same thing; the *iris* being no ways changed in that disease, any farther than that it is deprived of motion, from the sensation of the *retina* being destroyed by a palsy of the optic nerve.

The *nerves*, which are the source of all sensibility, are themselves of course extremely sensible. It is impossible to represent to one's self, without having seen it, the vast pain and disquietude which an animal is thrown into, upon touching, irritating, or even tying a nerve. I have learnt from experience that by tying a considerable branch, not only of the eighth pair, but even of the extremities, dogs have died after a few days; which has made me still more afraid than I was before of tying the nerves so common in amputations. But if a nerve is cut, and irritated below the section, the animal feels no sensation therefrom, which is a proof that pain is not propagated from one nerve to another by their anastomosing.

We have seen that the sensible parts of the body are the *nerves* themselves, and those to which they are distributed in the greatest abundance; for by intercepting the communication between a part and its nerve, either by compression, tying, or cutting, it is thereby immediately deprived of sensation, which is a fact proved by well known experiments, some of which you may see in my commentaries upon BOERHAAVE.[b] Wherefore the nerves alone are sensible of themselves, and their whole sensibility resides in their medullary part, which is a production of the internal substance of the brain, to which the *pia mater* furnishes a coat.

[b] De irritabilitate n. 284. not. g.

SECTION II.

I proceed now to irritability, which is so different from sensibility, that the most irritable parts are not at all sensible, and *vice versa*, the most sensible are not irritable. I shall prove both these propositions by facts, and at the same time I shall demonstrate, that irritability does not depend upon the nerves, but on the original fabric of the parts which are susceptible of it.

In the first place the nerves, which are the organs of all the sensations, are not at all irritable. This will appear surprizing, but at the same time it is very true. If you irritate a nerve, the muscle to which it is distributed is immediately convulsed. I never saw this experiment fail, and I have frequently by this means seen the diaphragm and abdominal muscles of a rat, and the hind and fore legs of a frog, thrown into convulsions. You may see experiments of the same nature in SWAMMERDAM, and in making them I have found, as OEDER [c] mentions, that the irritation of a nerve communicates the motion only to those muscles to which it is distributed, and does not effect those which derive their nerves elsewhere.

I have likewise constantly remarked, that the convulsion of a muscle only took place when the nerve was irritated with a knife, and not when the experiment was made with corrosives.

But if you irritate the nervous fibres which are scattered through a muscle, there is thereby no contraction produced in the nerve. I have often carefully observed in dogs, and more especially in frogs, that whatever irritation I gave to a muscle, it never communicated the least motion to the nerve.

I afterwards tried the same experiment which ZINN made at Berlin, viz. I applied a mathematical instrument, marked with very small divisions, lengthways to a long nerve of a living dog, in such a manner as I could thereby perceive the slightest contractions; and upon irritating the nerve, it remained perfectly quiet.

These experiments clearly prove, by the by, that the force of oscillation, which has been attributed to the nerves, is not agreeable to experience.

Neither the skin, which is the seat of feeling, nor the nervous

[c] De irritabilitate n. 22. p. 5.

membranes of the stomach, intestines, and *Urethra*, are irritable. Here we must take care not to confound with this property, a kind of vermicular motions produced by the corrosion which oil of vitriol, or spirit of nitre, communicates to the nerves, or arteries, when they are cut into pieces, or the contraction of the skin of the membrane of the *Urethra*, of the bladder, or gaul bladder, occasioned by the same application. The lungs have been constricted by oil of vitriol after death according to ZIMMERMAN.[d] And a motion has been observed some hours after death, in the skin, fat, and tails of animals.[e] Wherefore this corrosive force has nothing in common with life, and continues to take effect twenty four hours after death, as I have learnt by experience.

Neither is irritability proportioned to sensibility. The stomach is extremely sensible, and the intestines are rather less so, seeing they certainly are not liable to such violent pain, and yet I have found them to be more irritable. The heart, which is very irritable, has but a small share of sensation, and upon touching it in a living person, a fainting is thereby occasioned rather than pain.

Farther, we must not conclude that a part is sensible, because it is irritable; for the tying or cutting of a nerve, which destroys the sensibility of that part to which it is sent, does by no means destroy its irritability. I have frequently repeated BELLINI'S experiment, but the event was somewhat different from what is commonly related, viz. I laid hold of, and compressed the phrenic nerve of a living animal, or of one very lately dead, for the experiment succeeds alike in both cases; this compression irritating the nerve, put the diaphragm in motion; if I tied the nerve the same thing happened; if I cut it, and irritated it below the section, where there was no sensation left, because the communication with the brain was thereby destroyed, the diaphragm was convulsed the same as before. In the same manner by cutting the crural nerve of a dog, you thereby deprive the limb of all sensation, and you may tear it as much as you please without putting the animal to pain; but at the same time if you irritate the nerve which has been cut, the muscles of the leg are seized with a trembling motion; wherefore at that time it is irritable, though quite insensible. This experiment of BELLINI has

[d] De irritabilitate p. 17.

[e] *Ibid.* p. 13.

been too much embellished. It is true that the compression and irritation of the nerve puts the diaphragm in motion, but this is equally the case whether you press the nerve from above downwards, or from below upwards. The experiment succeeds better if the nerve is tense than if it is relaxed. If you compress the nerve, and irritate it above the compressed part, in whatever manner you do it, the diaphragm remains without any motion, and therefore it is not true what FREDERICK ORTLOB [e] has said, viz. that it is put in motion if you direct the compression of the nerve downwards, and ceases upon sliding the finger towards the upper part of the *thorax*.

Lastly, in small animals I have tied the trunk of the nerves which go to the extremities, and thereby rendered the limbs insensible and paralytic. Afterwards I have irritated the muscles, and seen them contract the same as before, though they were no longer subject to the command of the will.

I have tried experiments of the same kind upon parts separated from the body. The intestines in this state after being deprived of all communication with the brain, preserve their peristaltic motion; and if you touch them with a knife or corrosives, they put on the same appearances, as if they were in their natural situation, and still preserved their connexion with the nerves and brain.[f] The same thing is to be observed in the heart, and in any other muscle cut off from the body.[g] In an eel, the heart continues its motions during the space of several hours, with the greatest regularity, even after it has been pulled out of the *thorax*.

If therefore we say that an animal only feels, when any external impression is represented to the mind, certainly that part of the body must be void of sensation, whose communication with the brain is destroyed by the nerve being cut, or the part being taken quite out of the body. In asserting that there was no motion of our body but by the soul, Dr. WHYTT has found himself obliged to admit the divisibility of the soul, which he believes to be separable into as many parts as the body.[h] I have frequently repeated the experiment just now mentioned: *viz.* I pulled out the intestines as quickly as I could, and cut them into four or eight pieces, all which moved

[e] In præf. ad *anatomen rationalem* DANIELIS TAURY.
[f] WOODWARD'S supplement, pag. 76.
[g] ZIMMERMAN, pag. 19.
[h] L. C. pag. 383.

separately, still preserved their peristaltic motion, and contracted in whatever manner they were irritated. Dr. WOODWARD [1] tried experiments of the same kind before in the intestines, and BAGLIVI [k] upon the heart of a frog, as did AURELIUS SEVERINUS [l] long before the others. I have seen the heart divided into several small pieces, each of which moved upon the table. M. LUPS [m] has observed in the membranes of an egg an irritability which cannot proceed from any nerve, because there is no such thing there; but I have had no experience upon this article. I find that BAGLIVI [n] has made use of the same arguments to establish the seat of irritability in the solids, and we ought to take care not to call in here the analogy of insects, which are both irritable and sensible all over.[o]

The soul is a being which is conscious of itself, represents to itself the body to which it belongs, and by means of that body the whole universe. I am myself, and not another, because that which is called I, is changed by every thing that happens to my body and the parts belonging to it. If there is a muscle, or an intestine, whose suffering makes impressions upon another soul, and not upon mine, the soul of that muscle or intestine is not mine, it does not belong to me. But a finger cut off from my hand, or a bit of flesh from my leg, has no connexion with me, I am not sensible of any of its changes, they can neither communicate to me idea nor sensation; wherefore it is not inhabited by my soul nor by any part of it; if it was, I should certainly be sensible of its changes. I am therefore not at all in that part that is cut off, it is intirely separated both from my soul, which remains as entire as ever, and from those of all other men. The amputation of it has not occasioned the least harm to my will, which remains quite entire, and my soul has lost nothing at all of its force, but it has no more command over that amputated part, which in the mean while continues still to be irritable. Irritability therefore is independent of the soul and the will.

These same experiments farther prove, that the whole force of the muscles does not depend upon the nerves, because after these have been tied or cut, the muscular fibres are still capable of irritability and contraction; and some time or another perhaps, the use of the

[1] L. C. pag. 80.
[k] De fibra motrice pag. 7.
[l] Vipera pythia, pag. 119.
[m] L. C. pag. 34.
[n] De fibra motrice & morbosa, p. 7.
[o] Theolog. des insect. t. 2. pag. 84, 85.

nerves with regard to the muscles will be reduced to convey to them the commands of the soul, and to increase and excite that natural tendency which the fibres have of themselves to contract, in whatever manner that is brought about.

But I return to my experiments, whereby I have discovered which are the irritable parts of the human body, and in what degree they are so.

I have excluded the skin. The cellular membrane and the fat, which the oil of vitriol devours so greedily, is generally agreed to be void of motion, at least a gentle irritation does not affect it. So neither the lungs (tho' violent acids constrict them) the liver, the kidneys, nor the spleen, have any irritability; because they are composed of the cellular substance, which, as I said just now, is not endowed with it, and of vessels which are equally void of irritability.

This character of irritability seems to me to be that which distinguishes the cellular fibres from the muscular, which resemble each other so much, that they are confounded together every day, as appears for example in the *dartos*, which so many still look upon as a muscular membrane, as also in GLISSON'S *capsula*, and the round ligament of the womb, in which a great number of anatomists have been at a vast deal of pains to discover muscular fibres.

The disposition of the cellular membrane to irritability, is precisely the same with that of the fibres of dead flesh; it yields to the touch, dimples if you press it, and recovers itself if the pressure is removed. If you cut it, the fibres retract on both sides, and leave an empty space between. But the muscular fibres, if you irritate them in a living body with a knife or corrosives, become shorter; their extremities approach nearer to one another, presently they are relaxed again, and these contractions and relaxations are repeated alternately for some time.

The *tendons* are as void of irritability as they are of sensation. No irritation made with a knife or with any gentle corrosive can excite any convulsion in them, or produce any motion in the muscle to which the irritated tendon belongs. If the electric spark produced from the *tendon* is violent, the celebrated M. JALLABERT has observed, that sparks of the same kind may be produced equally as violent from other parts of the body which are more solid and hard.

The ligaments, *periosteum, meninges* of the brain, and all the membranes, being composed of the cellular membrane, are void of irritability: and the following experiment may serve to convince those of their error, who have fancied that they have observed muscular fibres in the *dura mater* and *pericardium*; *viz.* if you cut, burn, or tear those membranes, you can observe no sensible motion excited in them thereby. I have repeated this experiment an hundred times, and the same has likewise been done by Mess. ZINN, WALSTORF, CASTEL, OEDER and others, always with the same success.

The *arteries* by some are thought to be irritable, both upon account of their muscular membrane, and the necessity of finding the cause of their contraction, which is constantly alternate to that of the heart; and we know that Drs. SENAC and WHYTT [o] have looked upon this irritability as essential to the arteries. The first of these authors takes it for a more efficacious cause of the circulation, than the heart itself; and I own that this system has an appearance of reality. The intestines, which by their peristaltic motion push on their contents; the principal artery of the silk worm, which performs the office of a heart; and animals in whom after the heart has been cut out, the motion of the fluids has been continued for some time, which appears to be owing entirely to the arteries; lastly, local inflammations occasioned by irritating topics, form so many analogies, which unite to render this system probable. Upon examining with the microscope the blood in a fish, and in a frog, after the heart was pulled out, it continued for some time to move in the vessels, and I saw it pass up and down the vessels of a small fish, which had no motion neither in the heart nor gills, nor shewed the least sign of sensibility.

In the mean while all those facts do not yet prove the irritability of the arteries. Irritate the *aorta* of any animal you please, either in its internal or external surface, and either with a sharp instrument, corrosives, or hot spirit of nitre, you will not be able to observe any contraction thereby produced, except that from oil of vitriol, which I mentioned before,[p] and which succeeds equally as well, if the experiment is made several hours after the animal has been quite dead.

[o] L. C. p. 95.

[p] I. G. ZIMMERMAN p. 24.

In living frogs I have frequently irritated the arteries with alcohol, spirit of nitre, and other acrid liquors; and though I examined them attentively during the time with a microscope, I could never discover any motion thereby occasioned in them, while the blood which they contained was changed into a thick jelly of the colour of earth.

Farther, upon examining the circulation in animals with the microscope, I have never observed any contraction in the arteries. I have viewed the circulation for whole hours in fishes and frogs, and during the whole time the sides of the vessels remained as quiet as those of the tube with which I examined them. If the pulse of the artery had occasioned any motion in the neighbouring vein, I could not have failed to discover it with the microscope. With regard to the experiment which HEYDE [q] relates, *viz.* that upon cutting an artery of a frog, it contracts itself so as to become impervious, I have frequently seen the event very different, the orifice of the artery retaining its former figure, without being either contracted or dilated.

Wherefore, although I will not absolutely deny the irritability of the arteries, yet I do not at all see that it is confirmed by experiments.

Neither am I more convinced that it obtains in the *veins*. It is true I can perceive a motion in them, which depends upon respiration, and I have frequently observed, especially in frigid animals, that of the *vena cava*, which contracts near the heart, and drives the blood which it contains into the auricle. We likewise know that upon touching the veins with some very sharp corrosive, as spirit of vitriol, or hot spirit of nitre, they contract much more sensibly than the arteries, and push on the blood, as I have seen both in a kid, and also in a cat. But as neither the knife, nor gentle corrosives injected, produce such a contraction, and there is no acrimony of that force in any of the humours of the human body, I look upon the veins as intirely void of irritability, or at least very gently endowed with it.

The *lacteal vessels* contract and empty themselves, upon being touched with oil of vitriol, and what proves that they have a considerable share of irritability is, that though they are ever so full of chyle at the time when the animal dies, they empty themselves, and contract in such a manner, that you cannot discover any cavity remaining.

[q] Obs. 35.

The different *excretory ducts* are not endowed with more irritability than the veins. The gall bladder, the *ductus choledochus*,[r] the ureters, and the *urethra* contract themselves upon being touched with a very acrid corrosive, but the application of a weak one, or the knife, produces no change in them.

The ureters cannot be irritated even by oil of vitriol, and therefore they seem to be destitute of any muscular force, as indeed there has never been any muscular fibres plainly shewn in those canals.

I have convinced myself by an experiment of the nature of the bladder. For by pricking it with a knife or needle, in a dog that was almost dead, I have seen it, though not always, very frequently contract itself considerably, and drive out the urine through the opening of the abdomen. I have likewise observed it immediately after death contract itself spontaneously, and empty itself of all the urine with which it was full before; which observations were formerly made by WEPFER,[s] and I have quoted them before from him.

The watering of the eyes, which irritating *effluvia* or snuffs produce, and the discharge of *mucus* from the *urethra*, occasioned by sharp injections, together with other *phænomena* of the same kind, prove that the glands and mucous sinuses in man are irritable; but in the experiments which I have tried upon living animals, I have not discovered any such property in these parts.

The *uterus* in quadrupeds is irritable, and at least as sensible of motion as the intestines, whether it still remains in the body, or has been cut out of it. The strong contraction of the human *matrix*, which in a great measure expels the child, and is so evidently felt by the hand when it is introduced into it, proves its irritability, and this determined RUYSCH to leave the delivery of the after-birth to nature, as every body knows.

The Irritability of the genitals seems to be of a particular nature, in so far as voluptuous ideas are the most proper *stimulus* to put them in motion. However so far it resembles that of the other parts, as it is rendered active and produces an erection, if it is excited by a quantity of urine, or *semen*, the acrimony of cantharides, or the sharp discharge of a *gonnorrhœa*. But in whatever manner these irritations are produced, the effect of them is always to constrict the

[r] I. G. ZIMMERMAN 46 ab oleo vitrioli. [s] De cicuta aquat. p. 250.

veins, and to retard the motion of the blood through them. As Dr. WHYTT ascribes the erection of the *penis* to a greater afflux of arterial blood, he seems to be unacquainted with the experiments, whereby it appears, that by tying the *penis* either in men or brutes, an erection is produced, and that in the *paraphimosis*, the stricture of the prepuce occasions a prodigious swelling of the *glans*, though a greater afflux of arterial blood cannot be suspected in either case.

But all the *muscles* are irritable. I do not know one that has not a natural palpitation after death; they all tremble, and are alternately contracted and relaxed. I have observed it in the temporal, pectoral, and sternocostal muscles, in the *recti abdominis*, *cremaster*, and *sphincter* of the *anus*. Dr. WHYTT [t] has observed the same in the *sphincter* of the bladder, and other anatomists in other muscles of the body. With regard to the sternocostals, I have often seen with pleasure, that upon cutting away the *sternum*, they still preserved a sufficient force to bend the cartilages of the ribs, and pull them inwards. The same muscles preserved their Irritability a long time, nay even longer than the diaphragm. Farther, as the flesh in general of animals palpitates spontaneously after death, so it appears from an old experiment, which is known to every body,[u] that this motion may be revived after the muscles have been some time quiet, either by irritating the nerve which goes to a muscle, or the muscle itself, with a knife, or corrosives. Dr. ZIMMERMAN [x] has made experiments of the same kind with mine, as also Dr. WOODWARD [y] upon the muscles of oxen, CROONE [z] upon a muscle of the human thigh, and BREMOND [a] upon a frog; and OEDER [b] observed the muscles to be violently convulsed upon being touched with salt. In making the former experiment it is of little consequence, whether the nerve is intire and communicates with the brain, or whether it has been cut through.[c] In both cases the muscular fibre contracts, its extremities

[t] P. 93.

[u] HIGHMOR, *disquis. anat.* p. 137. B. LANGRISH *de mot. musc.* p. 51. WOODWARD p. 74. l. c. PARSONS *de mot. musc.* p. 68. W. CROONE *de mot. musc.* p. 10. MAZINI *de mechan. medic.* p. 13. HUGHES *of Barbados* p. 309.

[x] P. 19.

[y] P. 73, 74, 75, 76.

[z] *De mot. musc.* p. 30.

[a] Mem. de l'Acad. des Sciences 1739, p. 476.

[b] P. 2.

[c] CL. OEDER p. 5.

approach nearer to one another, and the succession of these motions represents a kind of undulation on the surface of the muscle. On examining in a frog with a microscope the muscle thus convulsed, you do not see that the blood is pushed out; and it circulates as before. There is no animal in which the muscles grow pale during their action, and I have long ago observed, that the paleness which HARVEY [d] said he had observed in the heart during its contraction, has been a source of errors into which the greatest men have fallen.

In most part of the muscles the Irritability is so great, that after one single irritation, the muscle contracts and is relaxed several times, by oscillations which diminish gradually, till they intirely cease.[e] This alternate motion is very evident in the *recti* muscles of the *abdomen*, the sternocostals and others, where there is no difference in the direction of the fibres, which therefore HAMBERGER,[e] and another author, had no manner of occasion to introduce into the structure of the heart, seeing the muscles just now mentioned oscillate perfectly well, though all the fibres are straight and parallel to one another. In the mean time Dr. WHYTT [f] is mistaken when he says, that this oscillation takes place in all the muscles, for it does not happen in the bladder, which after it begins to act, continues its contraction without interruption till all the urine is expelled.

The *iris*, which you will be surprized at, has no Irritability, when the cause of the irritation applied to it is mechanical, *viz.* a knife or the like, as I said before. Now that I mention the *iris*, I have observed, contrary to Dr. WHYTT'S [g] opinion, that its dilatation does not depend upon muscular force, because after death it remains very large. This I had remarked before a great many times, and I have a proof of it before me in a cat which was tortured to death, and has the pupil opened so wide, that you can scarce see any part of the *iris* at all. In a frog it also appears to be void of Irritability.

There are some muscles which have a stronger contracting force than others, and retain it a longer time after the animal is dead. The chief of these I reckon the *diaphragm*, which I have always observed to continue to move a long while after the others when the animal was dead, or at least by irritating the nerves the motions

[d] *Comment.* BOERH. *n.* 400. *Phys. prim. lin. n.* 4.
[e] WHYTT pag. 18.
[e] In progr. *de causa dilat. cordis.*
[f] Pag. 243.
[g] Sect. 7.

could be renewed. I have seen it irritable and tremble an hour or more after death, when the motion of the intestines had intirely ceased; and Dr. ZIMMERMAN [h] has been witness of the same along with me. WEPFER [i] has observed long ago, that the diaphragm contracted after the stomach was cut out. At the same time I must own, that I have sometimes seen in animals while they were still warm, other muscles, and the œsophagus continue their palpitations, after the heart was perfectly quiet, an example of which is related by OEDER.[k] But for the most part the diaphragm, heart and intestines may be irritated, or the heart and intestines palpitate spontaneously, after the motion of all the other parts is quite ceased.

The *œsophagus*, if it is irritated above the diaphragm, contracts itself very sensibly, and I have plainly seen its peristaltic motion without any irritation, and a morsel contained in it pushed upwards and downwards, by the peristaltic motion renewed upon irritation, which I think is sufficient to refute the opinion lately advanced against the motion of this muscular tube.

The *stomach* is considerably irritable, and when touched with a corrosive, its impression immediately produces a long superficial furrow in it. If you irritate it with a knife, either at the *pylorus* or elsewhere, it presently contracts; and I have seen it, especially upon touching the left side of the *pylorus* with a corrosive, contract itself circularly. If after you have opened it, you touch it with a corrosive, it throws out a froth, and the borders of the wound are rolled together,* as do those of the intestines. I have likewise seen the stomach shining through the diaphragm while the *abdomen* was intire, agitated with a peristaltic motion, and the same may be observed upon viewing it through the *peritonœum* after it has been laid bare, which contradicts the opinion of SCHWARZ, who attributes that motion to the access of the external air. I have seen it very evidently in a cat, a rabbit, and a rat, continue more than an hour after that of the intestines had ceased.

There is however in the stomach something less active, compared with the intestines; and upon irritating it in a frog with corrosives, it does not at all contract. I have frequently given these substances to them, and never but once saw them produce those motions which

[h] P. 19.
[i] *De cicut. aquat.* p. 195.
[k] De temporali p. 4.
* Et revolvit labia vulneris.

occasion vomiting, and consist in strong and short succussions repeated from time to time. Another time I saw the corrosive sublimate constrict the stomach, and make it perfectly flat.

The *intestines* both large and small, and the *cœcum* in those animals where it is large, are extreamly irritable; for after having cut the muscles of the abdomen entirely away, I have seen the excrements driven out by the force of the intestines alone, as WEPFER and STHAL[l] have observed before. We may add to these facts, so contrary to the system of those who attribute too much to the contraction of the abdominal muscles, that in the case of an obstinate costiveness, when the excrements resist the most violent straining, and repeated efforts of respiration, there is often nothing more required to expel them, than rousing the Irritability of the intestines by a glister. There is no part in the animal body which continues longer to move, frequently longer than the heart, as I have observed fourteen times; and when the heart moves the longest, it appears to me to be owing to the *abdomen* being first opened, and the intestines thereby rendered cold.[m] Nevertheless the brisk and perpetual motion of the heart, together with other reasons, give it the preference to the diaphragm. Opium, which destroys the peristaltic motion of the intestines, and almost the whole Irritability of the body, leaves the force and motion of the heart intire, as I have frequently seen. In several experiments, the motion of the heart has continued longer than that of the intestines, whereof I find seven examples in the journal of my dissections.

Very often after the motions of the intestines have ceased, they are renewed again, and increased by little and little, either by the cold or some obscure cause irritating them. Besides, after they have been taken out of the body, I have observed this motion rather to increase, which according to the common received opinion ought to be totally destroyed, and Dr. FELIX,[n] my pupil, has already made this remark. They may likewise be irritated externally either with a knife, a needle, alcohol, or corrosives; but their internal surface is much more irritable. Upon opening the intestinal canal, and dropping a corrosive into its cavity, the byle descends and flows out with

[l] Theor. vit. & mort.
[m] Comp. OEDER p. 5. & I. G. ZIMMERMAN.
[n] De motu peristaltico n. II.

a great deal of froth, and is alternately resorbed into the intestine. I never saw the peristaltic motion more plain than in a cat, which had swallowed corrosive sublimate. The lips of the intestine where it is cut through turn back, so as to embrace the part immediately above the section, and thus being turned inside out, readily adhere to the neighbouring parts. When the intestine is only slightly wounded the lips are equally retracted.

But it is so difficult to observe the peristaltic motion, that it is very hard to reduce it to any certain rule. In general however the intestines are evidently constricted where that motion obtains, while the part below the constriction is dilated, and receives the contents which the constricted part sends to it. Thus, when a part of the intestine is irritated, it contracts so strongly there, that the cavity is quite shut up, and the contents are pushed into the neighbouring parts, either upwards or downwards, which dilate, and soon afterwards, in consequence of this dilatation, contract themselves, and push along whatever is contained in them.

I have seen an introsusception in a rabbit, which had poison given it. In this case the contracted part of the intestine is received into the neighbouring part which is larger, and it is easily disentangled; but in the mean time it squeezes the contents equally upwards and downwards. It is likewise certain, that the intestine changes its situation from side to side, moving itself alternately from the left to the right, which motion renders the longitudinal fibres very conspicuous, as the constriction does the transverse.

In frigid animals the intestines appear to me to be proportionably less irritable. An hour after having opened the belly of a frog, I still found the stomach and the intestines to be irritable, but the motion of the heart continued a good deal longer.

Thus by degrees I am at last got to the most irritable organ of all, *viz.* the heart, which as it is the cause of all motion in the human body, so is the best constructed for it, and therefore ought to be endowed with the greatest Irritability. That it is actually so, appears from experiments, especially in frigid animals, in which it is constantly irritable, and much more than the intestines. For first, in these animals it moves a great deal longer than any other part of the body, even after death, and sometimes for four and twenty,° or

° In a large viper CHARAS *de la theriaque* p. 43.

thirty [p] hours, or longer; but in animals whose blood is hot, it moves till the fat is rendered stiff by the cold, at which time the motion of all the muscles commonly ceases. I have observed in frogs, that generally the heart continues its motion from noon till pretty far in the night, but rarely till next morning. Farther, after the heart has ceased to move, its motion may very easily be renewed by external irritation, either with a knife, a needle, salt,[q] poison, and sometimes even with simple hot water, according to Dr. WOODWARD.[r] The auricle being irritated by poison, contracted itself several times successively, and I have seen the same thing happen in the heart. But very often in those irritations produced by poison, the motion which follows upon them is very short, oft enough local, and confined to the place alone which is irritated. The best method of renewing the motions of the heart, is to irritate its internal surface, and I have frequently succeeded by blowing into it, after having tried corrosives without any success; and the injection of other fluids, which have more consistence than air, produces the same effect. The motion of the heart is equally renewed, whether you inject water, or blow air into it, either by the *vena cava*, the *trachea arteria*, or the thoracic duct,[s] which last experiment I tried upon a dog. In short, it is sufficient if the air gets any how into the left ventricle, which experiment usually goes by the name of HOOKE's, and I have tried it myself frequently with success. This irritation of the internal surface of the heart, produces oscillations much more lasting, than those which are caused by irritating it externally, and they become weaker very insensibly. Neither does this irritation at all diminish the Irritability of the heart, whereas that which is produced by poisons, renders whatever part they touch almost incapable of motion.

It is difficult to determine which is the most irritable part of the heart. Anatomists have commonly preferred the right ventricle and auricle. But, if I am not mistaken, I have proved that in this respect the right has no advantage over the left, seeing the oscillations of this last continue longer, when the irritating cause has been applied longer to it than to the right.[t] It does not appear that the weight

[p] In a turtle I. CALDESI.
[q] OEDER p. 3.
[r] L. C. p. 52.
[s] WEPFER cicut. aquat. p. 29.
[t] See the essay published at the end of this treatise. [This refers to the Essay on the Cause of the Motion of the Heart omitted from the present reprint.—O. T.]

of the liquor which is made use of contributes to increase the irritation, because air produces the same effect as water, although it is near a thousand times lighter, and the heart of a *fœtus* beats stronger and quicker than that of adults, whose blood is a great deal thicker and heavier. Hence I conclude that the different weight of the blood has no great influence upon the motion of this organ; and the experiments just now mentioned, made with air and water, prove that there is no need of acrimony in the fluids to produce an irritation of the heart; though at the same time I must observe that acrimony increases it, as appears from the example of salt. But acrimony and irritation do not increase in the same proportion; for hot spirit of nitre, applied to the internal surface of the heart, produces no contraction.

If you ask me whence proceeds this greater Irritability of the heart than of the other muscles, I shall find it very difficult to answer the question. The nerves are not more numerous there than elsewhere, and they are rather smaller than those of the muscles of the eye. Dr. WHYTT [u] is of opinion that the nerves there are more sensible, and therefore can less resist any stimulus. But whence can this excess of sensibility in the heart proceed? Is it because the nerves are more exposed, and nearer the internal surface of the heart, and by that means nearer the irritating cause? Anatomy gives us but little light into this subject, unless we might take for example the auricles, which are both very thin and very irritable. What would induce me to adopt this hypothesis, is the great Irritability which is observed in the intestines, which have only few nerves, but these very much exposed. To convince ourselves how much this circumstance increases the sensibility, we need only examine the symptoms which arise, when the mucus of the bladder and *urethra* is abraded; and of the intestines, when upon being deprived of their mucus, their naked villous membrane oozes out blood. But it is difficult to support this system by anatomical facts; for so far from being able to demonstrate, that the last ramifications of the nerves are extremely exposed in the heart, we find it difficult to discover their principal trunks. Besides, of all animals an eel appears to me to have the heart and other muscles the least capable of irritation.

[u] Pag. 311.

From all these experiments collected together it appears, that there is nothing irritable in the animal body but the muscular fibre, and that the faculty of endeavouring to shorten itself when we touch it is proper to this fibre. From the same experiments it likewise follows, that the vital parts are the most irritable; the diaphragm frequently moves after all the other muscles have ceased, the intestines and stomach move still longer, and lastly the heart continues its motions after all the other parts are quiet. This furnishes us with a distinct character between the vital organs and the others, *viz.* the first, being extremely irritable, require only a weak stimulus to put them in motion, whereas the others, which are endowed with very little Irritability, are not to be moved but by the determinations to the will, or by very strong irritations, the application of which is capable of producing in them violent commotions, known by the name of convulsions.

I shall easily prove, that this power of producing motion is different from all the other properties of bodies.[x] Elasticity, which seems to have the most resemblance to the other, differs from it in this, that it is proper to dry fibres, in which state they are deprived of all Irritability, as easily appears upon drying a frog. Besides elasticity is the property of hard bodies, and Irritability of the softest. The polypus is so irritable, that though it has no eyes, the light affects it very sensibly. Gelatinous animals are extremely irritable, but far from being elastic. Dr. WHYTT[y] adds, that the motion of the heart ceases and is renewed spontaneously, which is not observed in any elastic fibre, and upon pricking steel with a needle you produce no irritation in it. Dr. BATTIE[z] has observed, that Irritability is less considerable in old subjects than in young, although the fibres of the former are more elastic than of the latter.

But the muscular fibres being composed of earthy particles and a glutinous mucus, it may be asked in which of these Irritability recides. It appears most probably to recide in the latter, because this when it is pulled endeavours to shorten itself; whereas on the contrary, dry earth never changes its figure of itself, and being extreamly brittle, when its parts are separated, they constantly remain

[x] ZIMMERMAN in addend. OEDER p. 7.
[y] P. 231, *&c.*
[z] De princip. anim. p. 34.

so. This opinion is strengthened by observing that children, in whom the mucus predominates, are much more irritable than adults; which is evidently proved from the quickness of their pulse, which vibrates one hundred and forty times in a minute, whereas in old persons the vibrations are not above sixty or sixty-five in the same space of time. Farther, the most solid and earthy parts of our body, *viz.* the bones, teeth and cartilages, are void of Irritability, and the most irritable parts are deprived of that quality, only by robbing them of their mucus by drying.

We should next inquire how this mucus, produced from insensible lymph, can become irritable. Dr. WHYTT, and the followers of STHAL, alledge that it is owing to the soul, which being sensible of the touch, contracts the fibres that are touched, and pulls them back, to prevent their being injured.

However simple this theory may be, and however commodious for disembarrassing us from several difficulties, yet it is not agreeable to the phænomena which are observed. For in the first place Irritability differs intirely from sensibility, and the most irritable parts are these, which are not subject to the command of the soul, which ought to be quite the reverse if the soul was the principal of Irritability. In the second place, Irritability continues after death, and parts quite separated from the body, and intirely insensible, after the heart is taken out, and the head is cut off, remain still irritable. There is nothing more common than to see the heart of a frog beat, and the muscles remain irritable, after the head has been cut off, and the spinal marrow taken out. Dr. WHYTT [a] shuns this difficulty with great address by saying, that the time of death is very uncertain, and that frequently an animal has life still remaining, after it has been looked upon as dead for some time; and this he proves by the example of persons who have been drowned, and those who fall into *syncopes*. But as it is certain that the seat of the soul is in the head, and that it has no command over the rest of the body, after the nerves have been cut or destroyed; and farther, as the Irritability remains intire after the head is lopped off, or the nerves cut through, it appears that this quality still subsists after the seat of the soul is removed, or its commerce with the body quite intercepted, and therefore it does not depend upon the soul. This is so

[a] P. 367, 389, *&c.*

evident that it is needless to add, that Irritability acts without the soul being sensible of it, and that it is not subject to the command of the will, both which are proved by the example of the heart. To avoid the consequences of this argument, the anatomists are obliged to introduce an insensible sensation, and involuntary acts of the will, that is to say, to admit contradictory propositions.

What therefore should hinder us from granting Irritability to be a property of the animal *gluten*, the same as we acknowledge attraction and gravity to be properties of matter in general, without being able to determine the cause of them. Experiments have taught us the existence of this property, and doubtless it is owing to a physical cause which depends upon the arrangement of the ultimate particles, though the experiments that we can make are too gross to investigate them.

Irritability is destroyed by drying, by the congealing of the fat, but especially by the use of opium in a living animal. This medicine destroys so effectually the peristaltic motion of the intestines and stomach, that it cannot be revived again by any irritation. I have seen it myself, and Dr. KAAU BOERHAAVE [b] has observed the same thing before. Once however in a cat I found opium not capable of subduing the peristaltic motion. It has the same efficacy in destroying the peristaltic force of the bladder; nay in a frog I have seen it destroy the peristaltic motion and irritability of the intestines, and the convulsive force of the nerves. Dr. WHYTT says, that it likewise destroys the Irritability of the heart, but that I could never discover.[c]

To conclude, Some celebrated authors having asserted that Irritability was a property unknown till now, and having given me the honour of the discovery, while others have neither looked upon it as new, nor really existing, I have thought proper to add a short history of it here. Some obscure experiments which offered themselves naturally have been known in all ages, and VIRGIL has observed the trembling of the flesh cut off from the body immediately after death, but I don't see that the ancients have tried any experiments to irritate the fibres and renew their motions. FRANCIS GLISSON,[d] who discovered the active force of the elements of our bodies, was the first

[b] In impetum faciente HIPPOCRATICO.
[c] P. 371, 372.
[d] De ventriculo & intestin. c. vii.

who invented the word *Irritability*, which he attributes to a *natural perception*, that is not accompanied with any sensation, and depends upon Archæus who is the framer of his own body.[e] He distinguishes it into two sorts, the one of which depends upon the external senses, and the other upon the internal appetite.[f] He likewise mentions some facts to prove, that this motion is produced independent of sensation, that after death the fibres contract upon being touched with acrid and pungent liquors; nay he makes this property so general, as to allow even the bones and human juices to be irritable.[g] He likewise distinguishes the different degrees, and amongst the rest that excessive Irritability, which BOERHAAVE called *prurient*.[h]

BELLINI[i] mentions a *natural contraction*, and explains mechanically how acrid bodies, which might irritate the fibres, are repelled by means of this property, and thence he accounts how irritating substances may occasion contractions of the muscles, accelerate the motion of the blood, occasion an inflammation, produce a revulsion, or some certain evacuation. But he does not confirm these opinions by any experiments. BAGLIVI[k] by his experiments has approached nearer to a discovery of it. He observed pieces of the heart without the assistance of any nerve, tremble and oscillate, and preserve their alternate motion of contraction and relaxation;[l] and he has remarked, that the muscular fibres contract themselves upon being touched, without the soul's having any share in it, or even being sensible of it.[m]

Since that time all the followers of STAHL have enlarged much upon the tone and natural contraction of the fibres, but they attribute it to the soul, and as they have always had an aversion to anatomy, they have made no experiments to illustrate this quality.

Dr. BOERHAAVE[n] has acknowledged an active force in the heart, and a latent principle of motion in the pieces of it which are cut; but as he attributes the cause of muscular motion to the nerves, this proves that he did not sufficiently know, that the cause of this motion was in the muscles themselves, that the nerves had no other office

[e] N. 6.
[f] N. 11
[g] C. 8. n. 1.
[h] *Ib.* n. 6.
[i] de *Stimulis* amongst his *opuscula* and in his book *de sanguinis missione.*
[k] *De fibra motrice & morbosa.*
[l] P. 7.
[m] P. 12.
[n] Institut. rei med. n. 187. not. I. II.

but to subject them to the will of the soul,[o] and that they could indeed increase or diminish it, but were not the cause of it, because it is extended far beyond the nervous system, and we meet with it in insects which have not even a head. We find a number of curious experiments upon this subject, in Dr. WOODWARD'S posthumous supplement, published by HOLLOWAY. Dr. STEWART[p] has likewise observed several very useful facts, and he formerly remarked, that the fibres preserved their irritability after the nerves were detached from them.

We find scattered in several other authors, a good many things relating to this subject, which seem rather to have been dropt accidentally than wrote on purpose.

In my commentaries upon BOERHAAVE'S[q] Institutions, published in 1739, I have expressed myself as follows, *Wherefore the heart is moved by some unknown cause, which neither depends upon the brain nor the arteries, but lays concealed in the very structure of the heart itself.* The nature of the thing obliged me to differ in opinion from my preceptor. Three years afterwards I published the following doctrine,[r] *viz. That all animal fibres when they were irritated contracted themselves, that this character distinguished them from those of vegetables, and that the perpetual irritation alone was the cause of the continuation of motion in the vital organs, while the animal organs ceased to act.* In my abridgement of the physiology[s] I have positively ascribed the motion of the heart to the force of a *stimulus*, and in the second edition, I have been more explicit on the Irritability of the muscular fibres,[t] asserting that it was independent of the nerves, and of every other known property. If any person denies the truth of this assertion, I shall be glad to be informed by him, what the property is upon which that motion depends. Since that time, numerous experiments made upon living animals, had convinced me of the truth of the doctrine above advanced.

It gave me no small pleasure to see, that almost at the same time Dr. DE GORTER[u] published several observations to the like purpose,

[o] *Ib.* n. 402.
[p] De mot. muscul. p. 13.
[q] Ad. n. 187. Instit. rei med. not. i. II.
[r] Tom. 4. pag. 586. ann. 1743.
[s] Ann. 1747. n. 113. pag. 51.
[t] N. 408, p. 252.
[u] Exercitat. de motu vitali.

and that the celebrated Dr. WINTER,[x] physician in ordinary to the house of Orange, in an oration *de certitudine in medicina practica,* attributed all the motions of the human body to the Irritability of the fibres, and the force of a *stimulus*. These two famous physicians have been followed by several others. Dr. KAAU,[y] nephew to the great BOERHAAVE, has made a great number of experiments, although with another view; and not long ago the celebrated Dr. WHYTT[z] has attributed all the motions of the human body to the force of a *stimulus*; but with this difference between him and the others, that he imputes Irritability to the soul, which feeling the impression of the irritation, occasions the contraction of the fibre. I cannot help complaining of this gentleman's want of candour, both with regard to myself, and to several others, seeing he never mentions me but when he wants to criticise me, and has adopted several of my ideas, without mentioning whence he had them. He has made but a small number of experiments upon dying animals, which he has published with a view to support his system, but they have not been often enough repeated to be established as certain, and some of them are contradicted by those which I have made.

Two of my pupils, Drs. ZIMMERMAN and OEDER, have hit upon the proper method of arriving at the knowledge of this property, both these gentlemen endeavouring by experiments to explain this quality of a fibre, as a law of nature resembling attraction, without entering into useless researches in theory. CASTEL has likewise confirmed the experiments upon sensibility. The deceased M. DE LA METTRIE[a] has made Irritability the basis of the system which he advanced against the spirituality of the soul; and after saying that STAHL and BOERHAAVE knew nothing of it, he has the modesty to assume the invention to himself, without ever having made the least experiment about it. But I am certainly informed, that he learnt all he knew about it of a young Swiss with whom I am not acquainted: who never was my pupil, nor is he a physician, but he had read my works, and seen some of the famous ALBINUS's experiments, and upon these LA METTRIE founded his impious system, which my experiments totally refute. For if Irritability subsists in parts separate

[x] Franeker 1746. fol.
[y] De impet. facient. HIPPOCRAT.
[z] Of vital motions, Edinburgh 1751. 8.
[a] L'Homme machine n. 18. 22.

from the body, and not subject to the command of the soul, if it resides every where in the muscular fibres, and is independent of the nerves, which are the *satellites* of the soul, it is evident, that it has nothing in common with the soul, and it is absolutely different from it; in a word, that neither Irritability depends upon the soul, nor is the soul what we call Irritability in the body.

SUPPLEMENT by the AUTHOR.

Having seen, since the publication of my essay on Irritability, M. LE CAT's objections to what I had advanced, in a paper which he sent to the Royal Academy at Berlin,[b] I thought it incumbent upon me to answer it in a few words.

I do not know what motive could engage this author, or M. DELIUS, to refute me before I had published my opinion. They have either laid hold of some *theses* of my pupils, or expressions which have escaped me in some letter to a friend. This is certainly the case with M. LE CAT, and both of them might have spared a great part of their criticism, if they had only waited till the publication of my treatise.

In my first essay upon this subject, I inquired if the *dura mater* or tendons were irritable, if they contract, when any external violence is applied to them, and if in effect they can act like muscles. This enters essentially into BAGLIVI's system, and I plainly aver the contrary. All the animals which I have examined, have the *dura mater* strongly attached to the *cranium,* and when it is detached from it, it is always void of motion. In vain do we attempt to irritate it with a knife, needle, or corrosives more or less gentle; there is thereby no commotion raised in the animal. The same is the case with the *pia mater* likewise. In M. LE CAT's experiment, the application of spirit of wine is scarcely felt in the *dura mater,* whereas it produces a very violent pain in the skin; which shews that the first has no sensibility when compared with the second. Upon irritating the *medulla* of the brain, and that of the spine, convulsions are thereby quickly produced; wherefore the cause of motion is in the *medulla,* and the *meninges* have nothing at all to do with it.

The second doctrine which I have advanced is this, *viz.* that

[b] At the end of an essay upon muscular motion.

wounds of the *periosteum,* tendons, ligaments and *dura mater,* do not hurt the animal, and are cured without any accident. The experiments which M. LE CAT has recourse to against me are of no signification. They are vague and inconclusive. He must produce patients where a tendon, ligament, or one of the *meninges,* has been indisputably and singly wounded, and where troublesome accidents have ensued thereupon. What he says of the *dura mater,*[c] is explained by the compression which is produced in the brain, in consequence of that of the *meninges.* When we separate the *dura mater* from the *cranium* with the finger, the animal complains, even a moderate compression of the brain makes it suffer, and if it is pretty strong, the animal falls asleep. In the patient named CLERMONT, whom M. LE CAT [d] mentions, the optic nerve was hurt, as he himself owns, and it is very difficult in a cursory dissection to know, whether in the one called COURVET, the nerves of the eyes, and especially those that creep in the bottom of the orbit, towards the temples, were really preserved. The spasm may have causes not to be penetrated into by our senses, and founded upon the most delicate structure of the nerves. The hysteric *tetanus,* and that in animals which have been poisoned, confirms this opinion, and the observation of M. LE CAT [e] proves absolutely nothing at all, seeing it does not exclude this disorder, which is too deep seated to be visible. The history of PERCHEPIE [f] ought not to be produced against me, seeing this patient had *pus* in the ventricles and under the *basis* of the brain, which is more than sufficient to occasion a *delirium.* In order to refute me, M. LE CAT must bring experiments such as mine; the *dura mater* irritated, and convulsions thereby produced; the tendons pierced and wounded, and the ligaments pricked or burnt, and followed by violent symptoms. But no such experiments can be produced; nature is too constant, and I have seen her act too often to be deceived in this point. Neither should the difference between man and brutes be here quoted against me; for if wounds of the tendons had any influence upon their motion, a kid, a rabbit or a dog would not be able to leap up on a chair, immediately after the *tendo achilles,* had been pierced, cut, or destroyed.[g]

[c] P. 113. [d] P. 115. [e] P. 118. [f] P. 119.
[g] See the thesis of Dr. CASTEL.

Lastly, I have said, that the tendons, *periosteum* and *dura mater* are all insensible. I am not the very first who has advanced this truth, and I have quoted observers, who having no system to defend, had remarked the same thing before me. M. LE CAT mentions no experiments against me here, but with regard to the *dura mater*.[h] He relates that one, named FLEURI, complained upon his pressing this membrane with a crotchet; and that another wounded patient, called MABIRE,[i] was sensible of the motion of a tooth-pick upon the *dura mater*, that he felt the application of spirit of wine,[k] and the action of the surgeon who washed the wound;[l] and consequently that the *dura mater* must either be almost cartilaginous, or ossified, in those subjects which do not seem to have any sensation in this membrane. It even appears by his own expressions, that he has seen examples of this insensibility.[m]

A thousand times have I scratched, burnt, and cut the *dura mater*, in I don't know how many different animals, without their ever complaining, and they have seemed to feel still less the spirit of wine, which is infinitely less active than the butter of antimony or spirit of nitre. Young animals have sucked, or lapt milk, during the tearing of this membrane, without seeming at all sensible of what was a doing. It is absolutely impossible to imagine that the *dura mater* should be cartilaginous, or almost bony, in young healthy animals. The same animals were very sensible when the skin was pinched or pulled, which they discovered by making a noise, and endeavouring to pull away the part. The experiment has been made upon animals fierce and impatient, such as cats for example, which become furious when they find themselves in danger and pain, nay, the same experiment has been tried upon man, and M. ELLER has confirmed the truth of this doctrine at Berlin, upon the *dura mater* of a man, which had been laid bare by a *caries*. If the wounded person mentioned by M. LE CAT felt the pressure, it was no more than brutes themselves are sensible of; they feel very well the detaching of the *dura mater*, and the finger resting upon it, as I just now remarked. It might not even be impossible for very penetrating remedies to act beyond the *dura mater*, as cold water and acids affect the nerves of the teeth,

[h] P. 122. [i] P. 124. [k] P. 129. [l] P. 125.
[m] Page 129. near the bottom.

through the nervous and bony substance which intervenes. But I have never observed this fact to happen, and I repeat it once more, that the *dura mater* being only a cellular membrane, becoming evidently such where it accompanies the nerves, and having no nerves itself, it cannot be susceptible of any sensation.

I have only one word more to add, which is, to entreat physicians and surgeons, to lay hold of all opportunities of instructing themselves with regard to the insensibility of the *periosteum*, tendons, ligaments, and membranes of the brain. Such opportunities must frequently enough occur, and after they have laid hold of a tendon with a pair of *forceps*, as I have done that of the *flexor* of the third articulation of the finger, they will find themselves emboldened to make experiments, which are neither attended with danger nor inconveniency to the patient.

Allow me to add here that M. LA MURE published in tome 1749, of the *Memoirs of the Royal academy of arts and sciences at Paris*, an essay concerning the motion of the brain, analogous to that of respiration, which was read the 7th of August 1752.[m] This of mine was read before the royal society at Gottingen, the 22d of April and 6th of May 1752. Hence you see that it was published previous to M. LA MURE'S experiments, and it is plain that mine are anterior to his, seeing they are quoted by him,[n] and I was entirely unacquainted with those which he had made. Envy takes a pleasure in laying hold of slight occasions of detraction, and an essay wrote in 1752, but published in the volume of 1749, might furnish occasion for it in this case; and it would not be agreeable to be thought to have copied after another, when a person has followed nature alone. BERN 10th *Sept.* 1754.

[m] P. 541.

[n] Memoirs of the academy 1749, page 542. I communicated them in a letter wrote to M. DE SAUVAGE the beginning of January 1752, who sent me an answer as follows, dated March 1, 1752. *The dog was trepanned, and we observed a very considerable motion of the brain, exactly like that which you described in your letter to me. In order to be certain whether it is the reflux of the blood which causes this elevation during the time of expiration, M.* LA MURE *opened more than ten dogs: In short we have found the case to be exactly as you describe it, and I am extremely obliged to you for communicating this discovery.*

MEDICO-HISTORICAL ACTIVITIES IN THE UNITED STATES

MARYLAND

JOHNS HOPKINS UNIVERSITY, BALTIMORE

Institute of the History of Medicine

During the academic year 1936-1937, the following courses will be given in the Institute:

INTRODUCTORY COURSES

1. The History and Ethics of Medicine. DR. OLIVER. Tu., 4-5, one hour weekly during the academic year at Homewood, Remsen Hall 101. A general introductory course that may be taken in two successive years.
2. Outlines of the History of Medicine. DR. SIGERIST. First and second quarters, Tu., 4-5. An introductory course, illustrated by lantern slides, with bibliographic demonstrations, chiefly for first-year students.
3. History of Anatomy. DR. TEMKIN. First quarter, Th., 2-3. A seminar course on the history of anatomy for first-year students.
4. History of Physiology. DR. TEMKIN. Third quarter, Tu., 4-5. A seminar course on the history of the main physiological problems. Intended chiefly for first-year students.
5. Use of the Library. DR. LARKEY. First quarter. Hours to be arranged. The practical aspects of the resources of the Welch Medical Library; a brief survey of the scheme of classification; study of the various types of periodicals, indexes and other reference material. Informal talks arranged for small groups, who will be taken through the Library.

ADVANCED COURSES

6. Research Seminar. DR. SIGERIST and staff. Th., 4-5, throughout the academic year. This course is intended to make the students acquainted with the research methods of medical history. This year's program includes a survey of the recent literature dealing with the history of mediaeval and Renaissance medicine, a discussion of medical bibliography and a study of medicine in Utopias from Plato to our days.
7. Hellenistic Medicine. DR. EDELSTEIN. Second quarter, Fri., 4-5. A seminar course in which medical texts of the Hellenistic period will be read and interpreted. A knowledge of Greek is not required.

Sociological Courses

8. Social Aspects of Medicine. Dr. Sigerist. Third quarter, Wed., 4-5. A course intended primarily for advanced medical students, in which the development of society and the medical profession in it will be studied. The course is intended to give the medical student a background necessary for the understanding of the modern social aspects of medicine.

9. Sociological Seminar: Medical Problems of the Social Classes. Dr. Sigerist. Fourth quarter, Wed., 4-5. A seminar course in which the social structure of society will be analyzed, and the medical problems involved will be discussed.

Study Groups

10. Study Groups in the History and Sociology of Medicine. Dr. Sigerist and staff, throughout the academic year. At the request of three or more students, the Department will organize special study groups to investigate and discuss definite problems of the history, bibliography or sociology of medicine.

The Hideyo Noguchi Lectures will be given in the fall by Professor L. C. MacKinney, of the University of North Carolina, on a subject of mediaeval medicine.

The Johns Hopkins Medical History Club is closely connected with the Institute. Four meetings are held each year. Further information may be obtained from the Secretary, Dr. Oliver.

The Social Problems Forum, a student organization, meets on two evenings every month in the Lecture Hall of the Institute for the discussion of social problems.

H. E. S.